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A NEW METHOD OF MEASURING THE TIMES AND EFFICIENCIES OF PHOTOGRAPHIC SHUTTERS.

By J. De Graaff Hunter, B.A. (National Physical Laboratory).

Read before the Optical Society on November 15th, 1906.

The work on which the following paper depends was undertaken at the National Physical Laboratory, to provide a test for photographic shutters. The resulting apparatus has been used to measure the times of exposure given by several different types of shutters, and these actual times have ranged from one four-hundredth of a second to two seconds.

There seems to be no reason why much shorter times than one four-hundredth of a second should not be measured by the same means; but the shutters under observation in no case gave a more rapid exposure with the lens of the apparatus. When the time is longer than two seconds, or even one second, one can actually time the shutter with a stop watch, with quite sufficient accuracy.

The optical principles on which the method depend are first briefly explained. An electric lamp with a long straight filament is set up in a vertical position. By means of a lens, an image of the filament is formed. A shutter is put over the aperture of the lens, and if this shutter is set and released, the image of the filament is formed, and exists for just the time during which the shutter is open. We thus have to determine the time for which the image exists.

For this purpose a disc is mounted on a shaft, so as to be free to rotate. It is put in such a position that the image of the filament is formed on it. A radial slot is cut in this disc. The light from the point of the image which is formed at the slot, passes through the slot, and is visible from behind the disc.

Now let the disc be turned through an angle. If the image is still existing, the portion of it opposite the new

position of the slot is now visible from behind the disc. In fact, as the disc is turned, different portions of the image become visible. Now, suppose that by means of the shutter the image is only formed for a short period, and let the disc be rotated uniformly, assuming for the moment that it is crossed by the image, when the image is formed. Until the image is formed nothing is visible from behind the disc. As soon as the image is formed the portion opposite to the slot at once becomes visible from behind the disc, and other portions of the image are exposed in turn until the shutter closes, and the image ceases to exist.

It will be easily seen that different portions of the image are exposed, if the time for which the image is formed is lengthened or shortened. In fact, while the image exists, it is exposed at a definite *rate* by the slot in the rotating disc. That is the same thing as saying that a particular length of image is exposed when the image exists for a given time. From a measurement of this length of image we can infer the length of time for which the image exists. In this way we reduce the timing of the shutter to the measuring of the length of a strip of light.

In practice a disc having a number of slots is used; this being necessary to ensure that the image should be exposed, as above described, whatever the position of the disc at the moment at which the image is formed.

The essential features of the apparatus employed in carrying out the optical method just described, are:—

(1) A lens of large aperture (say two inches) and short focal length (say five or six inches).
(2) A source of light in the form of a long strip.
(3) An opaque, circular disc, in which some radial slots are cut, mounted on a shaft which can be rotated at any speed within certain wide limits.
(4) A mechanical device for releasing the shutter under test at a definite phase of the position of the disc.

In the apparatus, made at the National Physical Laboratory, an electric light—a "linolite"—with a long, straight filament is used as the source of light. An image of this is formed by the lens, which is put in a position so as to give unit, or somewhat greater, mag-

nification. The circular disc is placed in the image plane, and only those portions of the image which cross one or other of the slots in the disc can be seen by an eye placed behind the disc.

There are ten equidistant radial slots in the disc, and the image must be long enough to extend always across at least two of these; that is, it must subtend at the centre of the disc an angle equal to twice the angle between

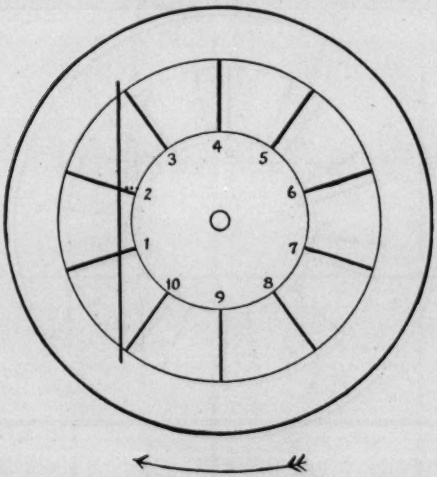


FIG. 1.

two consecutive slots. When the disc is made to rotate, different parts of the image become visible; in fact, if the disc turn with constant angular speed, a definite amount of image becomes visible in a given time. The slots all extend between circles of $2\frac{1}{2}''$ and $4\frac{1}{2}''$ radius, respectively.

Fig. 1 shows the position of the image relatively to the ten slots 1—10. When viewed from behind the disc, the amount of image shown, while the disc turns until the slots have the positions indicated by the dotted lines, is

shown in fig. 2. To see this, it is best to put up a piece of ground glass, close behind the disc, on which the image may be formed. If we can observe the angle subtended at the centre of the disc by the amount of image exposed by any slot, and if we know the mean speed of the disc, we can infer the length of time taken by the disc in turning through this angle.

Now, suppose we rotate the disc at a known speed, and

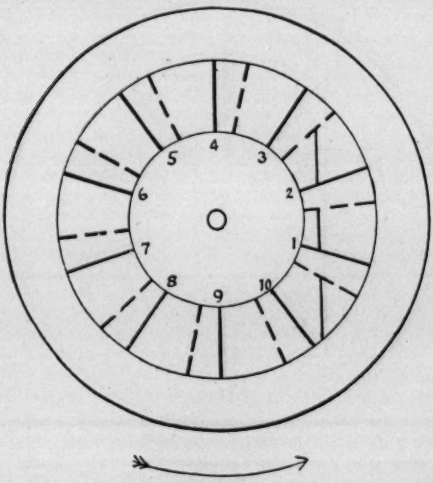


FIG. 2.

for a short time allow the light from the source to pass through the lens, we can find this time by measuring the angular length of image. Let θ° be this angle, n the number of revolutions of the disc in one second, t the time in seconds for which the image is formed, we have the relation—

$$\frac{\theta}{360} = nt \text{ or } t = \frac{\theta}{360\,n}$$

Now put up the shutter to be tested close in front of the lens. So long as the shutter uncovers any part of the lens, the complete image is formed; the lens having, if necessary, a stop placed in front of it to ensure this being the case. Thus, the time for which the image exists is the *complete* time of exposure given by the shutter over the lens aperture. This time is the "t" of the formula written above, and so can be determined by measurements of θ and n.

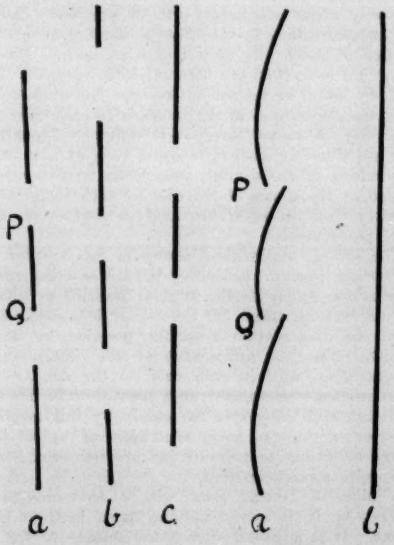


FIG. 3. FIG. 4.

To measure the angle θ, the positions of the two ends of the portion of image exposed by one slot of the disc must be observed, and we must avoid certain of the portions of the image. Thus, in fig. 2 we may measure the

portions exposed by slots 1 or 2; but that exposed by slot 10 is, obviously, not complete, as for the first part of the motion of slot 10 no exposure occurs. So it is a safe rule always to measure an inner portion of the image.

The image only exists for a short time—the time t—and it is, of course, impossible during this time to make observations of both ends. The time for which any part of the image is visible is simply the time for which persistence of impression on the retina continues. This is probably about one twenty-fifth of a second. All that can be ascertained in this time is whether an end of a portion of image lies on a cross-wire, or not. So it is clear that more than one observation is required.

If, by hand, we set and release the shutter more than once, the appearance of the image is in each case different. Fig. 3 shows three such different appearances. The positions we wish to measure vary on each setting and release of the shutter, though the whole angle subtended at the centre of the disc by each inner portion is every time the same, provided the shutter has a constant performance.

The several appearances shown in fig. 3 differ from each other, because the shutter in each case was released at random, as far as the angular position or phase of the disc was concerned. If the shutter is always released when the disc reaches a definite position, or is in a definite phase, the appearance of the image on the ground glass will, in each case, be the same. It is arranged for mechanically, in a manner to be described, that this shall always be the case. By this means the observer can have as many repetitions of the portion of image he wishes to measure as are necessary. All is now quite straightforward.

A piece of ground glass, with a cross-wire or line marked on it, is moved up and down in front of the image. It is adjusted after a few releases of the shutter to one end, P, of the image, P (fig. 3*a*), to be observed, and a reading on a suitably graduated scale is taken. The same is then done for the end Q, and the difference of the readings for P and Q gives the angular length of P Q; that is, θ. The speed of the shaft remains to be measured, and then t is known. The actual details for the measuring of θ are shown in fig. 7, and

described later. The whole process may be gone through very rapidly after a little practice.

When the exposure given by the shutter is anything shorter than one-twenty-fifth of a second, all the portion of the image exposed by one slot on account of its persistence on the retina, appears visible at the same time. This fact may be made use of to avoid measuring θ, and affords an alternative method of finding the shorter times of a shutter. The speed of the disc is increased until the image is *just* complete; *i.e.*, the portion exposed by one slot of the disc just joins up to the portion exposed by the adjacent slot. In this way the angle θ is known to be the angle between two consecutive slots in the disc. All that remains to be determined is the speed at which the disc is turning. However, there is some difficulty in adjusting the speed to just the right amount; and, of course, a number of releases of the shutter are wanted, with an increase of speed of the disc between each. Further, one has to be very careful not to increase the speed too much.

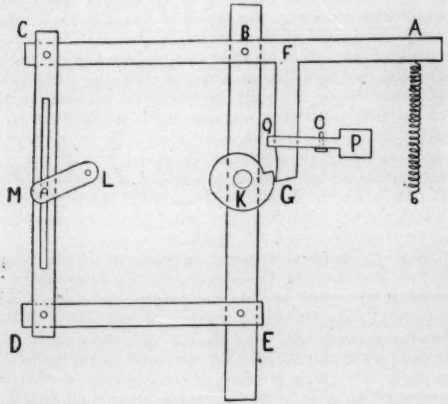
Fig. 4*b* shows the correct appearance of the image; but it is clear that the appearance is substantially the same if we speed up the disc further than the desired amount. One can make certain of not doing this by leaving a slight gap between the successive portions of image; when one is sure that the speed is not quite high enough. Or, it is possible, by continually moving the source of light, to make the image appear, as in fig. 4*a*, instead of fig. 4*b*. One can then be sure that the correct speed of rotation has been found. The shape of the curves in fig. 4*a* depends on the kind of motion given to the source. If this is moved with simple harmonic motion, these curves are portions of sine curves plotted against a tangent scale of abscissas.

At best it is rather difficult to see certainly when the image becomes complete, or nearly so, since the break may come at any position, and the continued straining to see if the breaks are just present or have just disappeared is very tiring to the eyes. For a few observations the method is sometimes useful, for times shorter than one-twenty-fifth of a second.

The appliance, with mechanical release of shutter, has been found much superior in this and other respects. It has been used successfully for finding various times of

exposure ranging from two seconds to one-four-hundredth of a second; the times being those given by one or other of several different types of shutters, including a focal plane shutter. It will be seen that the method does not depend on the period of persistence on

Fig. 5.



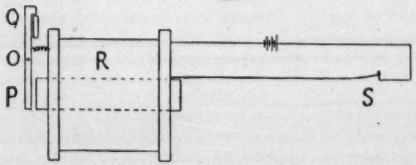


Fig. 6.

the retina, and so is applicable to times shorter than one-twenty-fifth of a second.

As far as I have been able to ascertain, all shutters may be released by a small motion of some accessible part. In many cases there is a trigger, while in others

a ratchet has to be released. In the mechanical device, above alluded to, for releasing the shutter at a definite phase of the revolving disc, a cam K (see fig. 5) is mounted on the same shaft which carries the disc, but at the end of the shaft remote from the disc. A system of bars, A B C D E B, is freely jointed at the corners B, C, D, E. Near B a bar F G is rigidly fixed to the bar A B C in a direction perpendicular to A B, and this bar F G works against the cam K. The bar B E is fixed to the bed plate of the whole apparatus. One end of a strong spring is attached to the bar A B at A, and this keeps F G in contact with the cam K.

The cam is of such shape that when the shaft has turned to a certain position, the arm F G drops over the abrupt angle of the cam, owing to the tension of the spring attached at A. This causes the bar C D to move upwards, sufficiently nearly in a straight line in a direction along its own length.

The bar C D has a long slot cut in it, and a bolt passing through this at M clamps the bar L M in any convenient position. It will be seen that the point L of the bar L M has adjustments in any desired directions in the plane B C D E, by compounding a motion of M along the slot, and a twist of the whole bar L M about M. A screw passes through the bar L M at L in a direction normal to the plane B C D E, and gives an adjustment in this direction. By these adjustments the end of this screw is brought into gear with the shutter release, and the upward motion of the rod C D then serves to release the shutter. This motion can be regulated to suit the particular shutter under test.

With the mechanism so far described the release of the shutter would take place at each revolution of the shaft. This was found to be very inconvenient when high speeds of shutter were being dealt with, as there was not sufficient time, after setting the shutter, to get ready to observe the image. To evade this, a little catch P Q (figs. 5 and 6) was made to hold back the bar F G, except when release was wanted. P Q is pivoted at O, and the end P is placed opposite the core of an electromagnet R. A little spring keeps it up against the bar F G, except when the electromagnet is in action, that is, when the key switch S is down.

The electromagnet is put at such a distance that it

exerts a force sufficient to overcome the little spring alone, but not this spring and the friction force between Q and F G. Thus it is only when the cam has lifted F G clear of Q that the catch Q is drawn aside by the electromagnet. This takes place when the portion opposite to the sharp angle of the cam K is in contact with F G, so that the arm F G is free to follow the cam when the angle turns round to G.

By the means just described the shutter may be set,

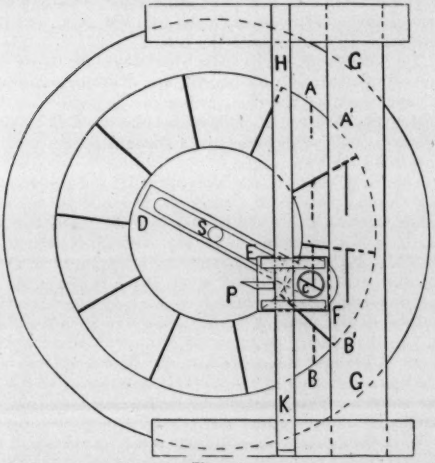


FIG. 7.

and the release can be made by pressing down the switch S, as soon as the observer is ready to make a setting for the end of the visible image.

Several ways have been thought of, and tried, for holding the shutter in position. It seems best to actually fasten the shutter down by screws to a wooden support with an aperture in it. This support is attached to the lens holder, and the shutter is brought as close as possible to the lens.

It has been found convenient in practice to work with

two images. A second source may be arranged for by putting up a second lamp, or two reflected images of one lamp may be used. This double source, however formed, is made such that the images formed by the lens are about one inch apart. Each image is observed from behind the rotating disc. One is formed on a strip of ground glass, and the other is used for "end" settings.

Fig. 7 shows the arrangement. The lines A B, A′ B′ represent the two images. A′ B′ is received on the strip of ground glass G G. Part of the image A B is observed on a small piece of ground glass, fixed at the end of a short tube, shown in fig. 7 as a circle C. The tube is mounted in a slide E, which moves up and down the bar H K. The ground glass on C has a line marked on it pointing to S, and the slide D E fixed to C, ensures this line always passing through S. On E is fixed a pointer P, and the angular position of the line on C is read by means of P on a properly graduated scale. The graduations give the position in decimals of a complete revolution.

Thus if the bar is set so that the line in C corresponds successively with the two ends of the visible image, the difference of the two readings gives the decimal of 360° subtended at S by the visible image, *i.e.*, it gives the fraction $\theta/360$ which came in the original formula, $t = \theta/360\,n$. It only remains to divide this by n, the number of revolutions per second of the disc, to find t the time of the shutter exposure.

In considering the quantity $\theta/360$, it is clear that θ cannot be measured in the way described if it exceed $360°/m$, where m is the number of slots in the disc. On the other hand greater percentage accuracy of setting can be made, when θ is as large as convenient. It is best to arrange the speed of the disc so that θ is about two-thirds of the angle between consecutive slots of the disc.

The use of the second image A′ B′ is to enable the observer at once to form an idea as to whether the speed of rotation is suitable to the time of the shutter. He also can see about where the ends of the image are. The shutters dealt with were often widely different in actual performance from nominal performance, and the time engraved on the shutter did not give much idea as to what was a suitable speed for rotating the disc.

Remembering that times of exposure to be measured vary between wide limits (say two seconds to one-four-hundredth of a second) it will be seen that is very necessary to have a ready means of altering greatly the speed of the disc. At first this was attempted by driving the disc by an electric motor and altering the volts on the motor by means of a variable resistance. In this way the speed could be varied considerably, but not nearly enough. To meet the case it was necessary to have a pair of coned pulleys giving speed ratios of 10 to 1 and 1 to 10 at the two ends. These were not connected by the ordinary crossed belt, but by a method devised by the author.

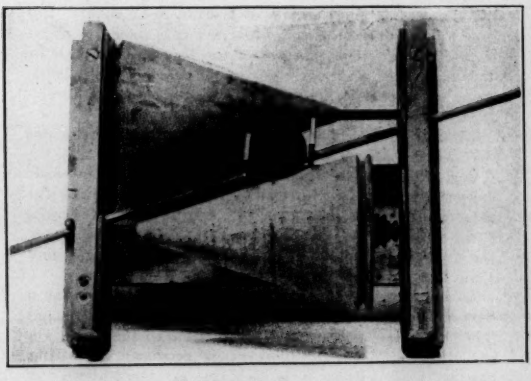


FIG. 8.

A rubber ball, merely resting between the two cones, transmits the motion from one to the other. The ball can rapidly be moved from end to end of the cones, giving the desired speed change. With this arrangement the cones can be used down to extremely small radii, where it would be impossible to have a belt drive. Fig. 8 shows the appearance of the system.

This, then, completes the apparatus. The general appearance is shown in fig. 9. It will be well to consider

to what inaccuracy the method is liable. The distance between consecutive slots where the image A B is formed is about 5 cm., and observations are made on a length of $\frac{2}{3}$ (5) cm. It is quite easy to make a setting within 1 mm. at either end, so that we have an error of .2 cm. in 10/3 cm., *i.e.*, a 6 per cent. error. The finite width of the slots of the disc perhaps introduce a similar amount. In taking the speed of rotation an error of several per cent. may be expected, and variation of speed of the motor introduces still further error. On the whole the author is confident that there can in no case be an error of more than 20 per cent of the quantity measured. A great many readings have been taken, and repetitions quite justify this as an outside estimate of possible error.

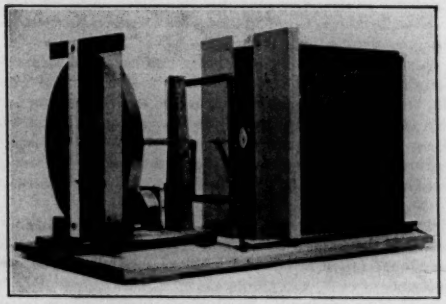


FIG. 9.

In a few cases a shutter has on different occasions given results differing by more than 20 per cent., but this has only occurred when the shutter has been acting in a variable manner. Of course, it cannot be expected that a shutter should have precisely the same performance from day to day, and after further wear.

As indicated in the title, tests for efficiency have been made with a number of shutters. What has actually been measured is the length of time during which the

shutter is open beyond a certain amount. In the cases of shutters which open from the centre, the method followed was to put up a stop as near the shutter as may be, and shaped like the aperture of the shutter. The shutter had then to open beyond this aperture before any image was formed. The ratio of the area of this aperture to the aperture when the shutter was full open, gave the amount of lens uncovered, when the stop was removed.

Stops were made to eclipse about $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ of the aperture, and in each case these stops were of the shape of the shutter when open to an equal extent. After readings have been taken and reduced, with the various stops in the way, it is possible to draw an efficiency curve in which abscissas give time during which the shutter is open, and ordinates give the aperture beyond which the shutter was open for this time. The efficiency is given by the ratio of the mean abscissa to the greatest abscissa.

Measurements were made on a number of shutters to test the accuracy of the method. Several of these were old, and the experiments must not be looked upon in any way as tests of the type of shutter, as no special care was taken to secure representative shutters. The object of the investigation was to test the method of measurement and the possibility of applying it to shutters of various forms.

Results are given which were obtained from four shutters. Of these the first two were diaphragm shutters, and the others roller blind and focal plane shutters, respectively.

In the case of shutter No. 1, the actual readings taken are reproduced so as to illustrate the method in which results are arrived at. These are given in Table I. Column I. gives the nominal times of the shutter; column II. gives the reading for one end of image minus the reading for the other end, *i.e.*, the angle subtended at the centre of the disc by the visible portion of the image, in decimals of a complete revolution; column III. gives number of revolutions of the disc per second, usually measured over ten seconds; column IV. gives actual time of shutter, being II. ÷ III.

Two independent sets of readings, A and B, were taken for comparison.

A

TABLE I.

Nominal times.	Readings.	Revs. per sec.	Actual time.
1	.142 – .067	.12	.63
½	.152 – .066	.19	.45
1/5	.145 – .067	.21	.37
1/25	.160 – .068	.34	.27
1/50	.132 – .092	2.0	.020
1/100	.172 – .116	3.6	.0156
B			
1	.124 – .066	.09	.64
½	.138 – .066	.17	.42
1/5	.142 – .067	.22	.35
1/25	.142 – .068	.35	.21
1/50	.164 – .110	3.0	.0180
1/100	.172 – .113	3.4	.0173

Here there seems to be good agreement of results. The worst difference is in the case of the 1/25 second nominal, where it is 25 per cent. of the mean value of the two results. This is, no doubt, partly due to the performance of the shutter not being quite constant. But the amount by which some of the times differ from the nominal times is far greater than the amount which might be attributed to inaccuracy of reading. Thus the nominal 1/50 is nearly correct, while the nominal 1/25 is really about 1/4.

When arrangements were being made for testing efficiency one of the leaves of this shutter was broken. This was afterwards replaced by a new leaf, after which the following results were found for the two fastest times. Three stops were made of proper shape, and these were placed close to the iris diaphragm. The three stops eclipsed ¾, ½, and ¼ of the aperture respectively. The results are in—

TABLE II.

Nom. time.	Stop 8.	Stop 2.	Stop 1.	No stop.
1/50	.0098	.014	.018	.022
1/100	.0102	.014	.017	.021

The corresponding efficiency curves are shown in figs. 10*a*, 10*b*, the efficiencies working out to be .64 at 1/50th second, and .66 at 1/100th second.

Shutter No. 2. TABLE III.

Amount of aper. eclip. by stop.	.70		.47		.26		0		
	A	B	A	B	A	B	A	C	D
1/300 ...	.0026	.0026	.0039	.0035	.0041	.0045	.0054	.0038	.0046
1/200 ...	.0030	.0032	.0041	.0043	.0048	.0048	.0058	.0057	.0054
1/100 ...	.0040	.0029	.0041	.0050	.0061	.0050	.0067	.0066	.0056
1/25 ...	.0050	.0040	.0043	.0048	.0072	.0065	.0081	.0078	.0073
1/10 ...	.0100	.0103	.0235	.0103	.0118 .0159	.0114	.0195	.018	.020
$\frac{1}{4}$ ...	.050	.037	.071	.042	.034 .050	.039	.064	—	.06
$\frac{1}{2}$ ...	.11	.09	.168	.102	.111 .149	.140	.157	.13	.15
1 ...	.21	.26	.224	.336	.247	.266	.296	—	—

Shutter No. 2.

The following Table III. shows the results found for shutter No. 2. Stops 1, 2, 3, were made of the proper shapes. Their areas were measured by planimeter, and were found to be .7, .47, .26 of full aperture, respectively. After one complete set of results (A) had been found, a second set (B) were taken for comparison, and two more sets (C and D) for full aperture taken at an entirely different time are shown for comparison with (A) and (B).

The A result for 1/10 with stop .47 is obviously due to either a misreading or a failure in the action of the shutter. The headings .7, .47, .26, 0 of the columns indicate the amount of aperture exposed for period of time indicated in the table.

TABLE IV.

MEANS OF TABLE III.

Aperture Eclipsed.	.70	.47	.26	0
1/300	.0026	.0037	.0043	.0046
1/200	.0031	.0042	.0048	.0056
1/100	.0035	.0046	.0056	.0063
1/25	.0045	.0046	.0069	.0077
1/10	.0102	.0103	.0130	.0192
¼	.044	.057	.041	.062
½	.10	.135	.133	.146
1	.24	.28	.26	.30

In these results it will be noticed that from one second nominal to 1/100th nominal, the nominal times are longer than the actual times. Thus—

1 sec. nominal is really 1/3
½ " " " " 1/7
¼ " " " " 1/14
1/10 " " " " 1/50
1/25 " " " " less than 1/100

The 1/25 nominal is only 1.7 times as long as the 1/300 nominal.

The performance of the shutter when set at speeds of ¼, ½, 1 sec. nominal does not seem to remain constant, and so the results worked out with a view to finding efficiencies at these speeds are contradictory.

The efficiency results for the faster speeds down to

1/10th nominal work out at 1/300, 68; at 1/200, 65; at 1/100, 64; at 1/25, 61; at 1/10, 55; showing a steady decrease of efficiency.

Roller Blind Shutter (No. 3).

In this shutter the lens aperture is exposed by a wide gap in the blind. Suppose d is the width of this gap and a the diameter of the aperture to be exposed, then the complete time of exposure is the time in which the blind moves a distance $d + a$. It is clear, then, that this time depends partly on the size of the lens aperture.

With this shutter two sets (I. II.) of readings were taken with the shutter working across an aperture of $1\frac{3}{8}$ inches. Results are shown in Table V.

TABLE V.

Nom. Times.	I.	II.	Mean.
1/12	.066	.059	.063
1/25	.045	.046 .048	.047
1/35	.037	.042	.040
1/50	.0325	.036	.034
1/70	.031	.032	.032

It will be seen that the agreement in the two sets of readings is fairly close.

In this shutter no correct statement of the time of exposure given can be made without at the same time the size of the lens aperture with which it is to be used. As before, if d is the width of the gap and a the diameter of the lens aperture, the shutter has to move through a distance $d + a$ while the lens is partially or wholly uncovered. It is clear that if a is altered the time of exposure is also altered. The most natural statement seems to be the time of the shutter in exposing a point. As d is large compared with a, we can assume some mean velocity v of the shutter without making much error. Then the time t with aperture of diameter a is given in terms of T the time over a point by relation $t = \frac{d+a}{d}\,T$.

Focal Plane Shutter (No. 4).

In testing this, the shutter was placed close to the lens, and the time of exposure given by the shutter with slit

of width d when passing over an aperture of width a was found. This is the same thing as the time for slit of width $d + a$ to pass over a point, if we assume uniform velocity over this small width. From this was inferred the time taken by a slit of width 1 cm. to pass a point.

Six sets of readings were taken: two (A, B) for a point near edge of the plate which would first be exposed; two (C, D) for a point near centre of the plate; and two (E, F) for a point near the edge of the plate, which would last be exposed. From the results it is possible to notice the changing speed of the shutter as it crosses the plate.

The results are in Table VI.

TABLE VI.

Setting.	A	B	C	D	E	F
1.	.0132	.0126	.0113	.0107	.0084	.0088
2.	.0120	.0105	.0064	.0078	.0061	.0075
3.	.0095	.0087	.0059	.0069	.0053	.0061
4.	.0082	.0084	.0056	.0066	.0049	.0051
5.	.0078	.0077	.0047	.0052	.0045	.0044

Column I. gives the setting of the shutter which was used, and the remaining columns give corresponding times. Pairs of readings A B, C D, E F, were taken for comparison, to see that the differences found for different positions was larger than that due to inaccuracy of reading. Comparing A with B, C with D, E with F, we see that in general the agreement is fairly close, and that the difference between the various pairs is a real one. This is put in a more convenient form in Table VII., where means of A and B, C and D, E and F, are given; also, the nominal times of the shutter.

TABLE VII.

Setting.	I. Edge first exposed.	II. Middle.	III. Edge last exposed.	Nominal times.
1.	.0129	.0110	.0086	1/50
2.	.0113	.0071	.0068	1/100
3.	.0091	.0064	.0057	1/150
4.	.0083	.0061	.0050	1/200
5.	.0078	.0050	.0045	1/250

It is clear that in position I. the shutter has not attained its full speed, at any of the settings; and with the spring at its weakest, setting No. 1, the full speed is not reached even by position II. This is also the case to a less extent with the other settings, but it is not supposed that the figures given have sufficient accuracy to distinguish certainly between .0050 and .0045, though the uniform run of the figures rather supports this view.

Between Columns I. and II. the difference is quite beyond the difference possible through error of reading, etc.

Some visual methods have been used for shutter tests in which a lens has not been employed to form an image of the various objects observed. In two of these the

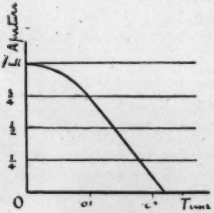


FIG. 10A.

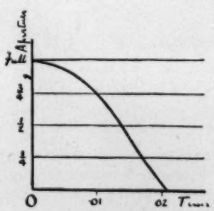


FIG. 10B.

shutter to be tested has been held close up to the eye. A little consideration will show that this cannot give a correct result except in certain cases. If the shutter is one which opens from the centre and then closes towards the centre the method may be correctly employed if the eye is placed so that it can see the object to be observed as soon as the shutter is very slightly open.

But a roller blind shutter is not correctly dealt with in this way. For, if the eye is placed at the centre of the aperture, it is only when the shutter has uncovered half the aperture that the eye can see the object to be observed; and after the shutter, when closing, has covered half of the aperture, nothing can be seen.

In other words the time which could be measured by this means would be the time for which a single point

TABLE VIII.—Showing ratio of actual times to nominal times.

Nominal time. 1	1/2	1/4 – 1/5	1/8 – 1/12	1/16 – 1/25	1/32 – 1/40	1/50	1/64 – 1/80	1/100	1/150	1/200	1/250 – 1/300
.63	.87	1.8	—	6.0	—	.95	—	1.6	—	—	—
.30	.29	.25	.19	.18	—	—	—	.63	—	1.1	1.3
1.2	1.4	2.9	—	7.8	—	1.2	—	1.7	2.7	—	—
1.0	1.5	2.4	—	6.0	—	.75	—	1.3	2.0	—	—
—	.94	1.6	—	7.3	—	.46	—	1.1	1.6	—	—
1.6	2.3	2.8	4.3	2.1	1.7	—	1.1	1.6	—	—	—
—	—	—	1.3	1.1	1.4	—	2.6	—	—	—	—
—	—	—	.76	1.2	1.4	1.7	2.2	—	—	—	—
—	—	—	—	—	—	.43	—	.68	.86	1.0	1.1

(placed where the eye is) receives light through the shutter. On the assumption of any law of velocity of the shutter blind it would be possible to deduce the time over a lens of given aperture.

In other papers on shutter testing results have been published which show that the shutters considered differ very widely in nominal and actual performance. And the same feature is very noticeable in the shutters with which the author has had to deal. In the following table (VIII.) the results of a large number of his tests on different shutters are collected. The entries made represent the ratio of actual time to nominal time. Each horizontal row refers to one shutter. The headings of the columns give a small range of time in which the nominal time of the shutter is included. In the case of two of the shutters which have a certain nominal time in common, one shutter gives an exposure of forty-three times the length of exposure given by the other. But the table explains itself.

Sir William Abney said the apparatus they had had the advantage of seeing before the paper was read, was very beautiful in design, and, so far as his knowledge went, was novel. It seemed to him to have been worked out with very great skill and care, and he was surprised at its accuracy and the small differences there were in the two or three readings in succession. He was glad that Mr. Hunter had called attention to the actual times and the nominal times, as no one knew, perhaps, better than he, that the ratios of the times put on shutters were in many cases absolutely fictitious. As a rule, the speeds merely indicated that the plate was exposed for a longer or shorter time, and nobody who knew this would ever think of using the figures marked on the shutters. That, at any rate, was his experience—he would mention no shutters and no names, but there were a great many sinners in that respect, but he had not the slightest doubt that they arrived at their figures by some means or another.

If they got one of the instruments now shown they would, at all events, have the correct nominal exposures to go by. A good deal of experimental work had been carried out by amateurs in measurements in which they had taken for gospel the makers' times of exposure. This was unfortunate, as it rendered such work of but

little use. Of course one had to recollect that with shutters the actual exposures might vary, for anybody who had used a shutter in which thin plates of ebonite formed a part would know that the exposures varied very considerably.

There was one shutter he had in mind with regard to which he could say the times had varied between 1/100th of a second and about 1/12th of a second, though they were supposed to be the same. The difference was due to change of temperature and deposit of moisture. So that one must not blame the makers altogether. He would not make any further observations except to congratulate Mr. Hunter on the very efficient apparatus he had introduced, and he hoped it would be found useful to the manufacturers of different kinds of shutters.

Mr. Conrad Beck said that some years ago he noticed that a certain shutter put on a certain camera was marked 1/100th, which a year before had been marked 1/50th, and without any very apparent change, the next year it was marked 1/150th. He was bound to say that he wondered where it was going to stop. At that time he commenced a scheme by which he tested customers' shutters for fourpence each, and he tested nearly 6,000.

In the course of this work, they got some extremely interesting information. He certainly could endorse everything Mr. Hunter had said as to the question of accuracy of speeds of shutters; at the same time he was bound to say there were many ways of looking at it. For instance, if a shutter opened from the centre and closed to the centre, the exposure which was required to be given, according to exposure tables, would not be in the same ratio to the total time the shutter was open as that required by a focal plane shutter. Consequently, there were two methods of defining the rapidity of a shutter, one of which had reference to the amount of light admitted into the camera and regulated the exposure necessary, the other had reference to the period during which a hand camera could be held steady, and to the movement of a quick moving object being photographed, and he thought there was some difficulty in expressing the speeds of shutters for that reason.

It was the simplest, and perhaps the more practical and certain method of expression, to state the actual period during which the shutter opened the central por-

tion of the lens. Then, again, one had also to consider the size of stop or aperture of the lens which was going to be used. If the total time during which a shutter was open, modified by an efficiency co-efficient, were taken as the speed of a shutter this might be quite incorrect, because the shutter might have a full aperture of 1-inch on which the efficiency was calculated, whereas the lens used with it had perhaps an aperture of $\frac{3}{4}$-inch. Mr. Hunter had been suggesting that his method was a fair test of a blind shutter, but it was not correct to take the total motion of the blind. A blind shutter working in front of the lens commenced by exposing one edge of the plate, the rest being in darkness, and this edge of the plate ceased to be illuminated some time before the shutter had completed its travel. Thus, a measurement giving the total travel of the blind did not give a correct measurement of the exposure at any one point of the photograph, and was not a useful mode of expressing the speed of that shutter. He merely raised those points with a view to showing that the question of expressing speed of shutters ought to be carefully discussed and set forth by some consulting body who would consider all those points, in order that some systematic method could be adopted. He thought there were two distinct points which every man wanted to know. Firstly, the speed of his shutter with reference to exposure. Secondly, how much the moving body would move during the exposure, and the probability of shaking the camera during the exposure. They were extremely difficult points, and it was very difficult to know what was the most convenient method of expressing the speed when two such dissimilar points had to be considered.

He thought Mr. Hunter was to be congratulated on his method of testing shutters—the change of speed and other details were ingeniously worked out. He must say that he should feel a great deal of satisfaction if, as a check to some of Mr. Hunter's results, they had one or two photographic tests—for instance, Sir William Abney's method—to test their accuracy. In starting any method, it was above all essential that there were no chances of errors creeping in. He was bound to say, judging from the average of his own results, he was a little suspicious as to whether Mr. Hunter's figures did

not give shorter exposures than should be the case. In a method he had himself adopted for testing shutters, which had to be a rapid method, the shutter was placed before the eye, a small stop being used and a series of bright spots rapidly uncovered by a revolving slit was observed.

Mr. Hunter's machine was a modification of the invention of Mr. Kershaw, of Leeds, which he thought was the most ingenious he had ever seen. That method was patented by Mr. Kershaw four or five years ago, and he used it for testing his focal plane shutters. For that purpose he did not think it could be much improved, and in discussing a question of this kind he thought credit should be given to Mr. Kershaw for his method.

Sir William Abney wished to add one or two words in reference to the time of duration in the method he adopted himself—the photographic method. Of course he always used the method with the lens which was going to be used—not without a lens, although it could be measured without a lens—the photographic diagram had no possible meaning unless it gave the mean time of the exposure on the shutter with a certain stop. With the diagram method there was none of that uncertainty which Mr. Beck referred to. There was the time before them, and they could calculate it as they liked. With a focal plane shutter one could not judge of the ordinary rules of shutters, because they had only got to alter the slit, and they got as rapid an exposure of a certain portion of the plate as they liked. But where they had the shutters near the lens, the diagrams they got by the photographic method left no manner of doubt as to the speed obtained. Mr. Hunter had got approximately the total time a shutter was opened. He did not get the mean time, but he got his diagrams in a different way.

Mr. Purser said it would be a good thing if the speed of shutters could be accurately measured by such an instrument as that which Mr. Hunter had devised, but the question of more importance was the production of a shutter that would do credit to the measuring instrument. Manufacturers are quite conscious of the fact that the speeds of shutters are only approximately correct, and the production of an elaborate instrument for testing does not bring us any nearer the perfect shutter.

In the production of such an article there are so many factors that affect the constancy of the working parts, such as changes of temperature acting on the springs of the shutter blades, grit or moisture on the valves, etc.; that, in the absence of constancy, it was not much satisfaction for the photographer to know that some time in the dim and distant past his shutter had worked at correct speeds, because at the present time, under changed conditions, it was working quite differently.

In recording incidents referring to the inconstancy in the working of shutters and other factors, Mr. Purser humorously asked the question, "Did we want speeds in shutters, and are they of any use when we have got them?" He recently saw some photographs taken by a camera having a shutter working at nominal speeds of 1 to 1/100th of a second. They were hunting pictures with horses and dogs scampering across a field, and the photographer recorded the information that they were taken at 1/25th of a second. It was generally considered by the amateur photographer that in order to take pictures of animals moving at a rapid rate across the field of vision a speed of at least 1/300th of a second was necessary. He also remembered conversing with a gentleman who had promised to lecture at a local photographic society on "Exposures," and "Rapid *v.* Slow Plates."

Just two days previous to the lecture he purchased a packet each of extra rapid and ordinary plates and made a few exposures. On developing the plates under the same conditions he found precisely the same results, the negatives from the extra rapid plates being quite indistinguishable from those of the ordinary plates, notwithstanding the fact that the exposure given had been precisely the same. Whether this result was due to lack of constancy in the working of the shutter or to other causes is not recorded, but the incident goes to prove that where so many factors are unreliable it must be difficult to arrive at satisfactory results, and he thought that an elaborate method of testing of shutters could not be of any serious service to amateur photographers until such time as a shutter could be produced that would give constantly reliable speeds.

Mr. Rosenhain said that he particularly admired the ingenious mechanical arrangements of Mr. Hunter's apparatus, and especially the change-speed gear; he only

wished it were possible to apply such a form of gear to the transmission of a considerable amount of power, but he understood from Mr. Hunter that there were very serious obstacles to its use, so that those who had looked at the gear and dreamt of noiseless motor cars should not be too sanguine. In the mechanism for releasing the shutter at the proper moment there seemed to him a slight duplication; could not the electro-magnet used to put the mechanism into action be employed to cause the release of the shutter directly?

Further, after looking at the very perfect arrangements of the apparatus, he (Mr. Rosenhain) was led to wonder that no greater degree of accuracy than that claimed by Hr. Hunter (20 per cent.) could be attained; on looking through the figures given in the paper, in fact, it appeared as though Mr. Hunter's claim were rather too modest. He supposed that the object would hardly justify such a refinement, but by the use of a chronograph, the speed of rotation of the disc, at all events, could be measured to a much higher degree of accuracy than the 6 per cent. mentioned in the paper. However, while he (Mr. Rosenhain) was thus asking for greater accuracy, others had said that no speed-testing of shutters was required at all—that these measurements were of no real value to the users of shutters. He did not at all agree with that; if he knew what certain shutters did under standard conditions, and how far they were liable to vary, that would be a means of discriminating between the good shutters and the bad, and would thus be the first step towards the attainment of the perfect shutter.

Even Mr. Hunter's results, as they stood, appeared to show that the roller-blind type of shutter was less liable to vary than the diaphragm shutters. As for the argument deduced from the fact that with the same exposure equally good results might be obtained upon fast and slow plates, that hardly bore on the question; they all knew that by suitable development good results could be obtained with enormous variations of exposure, but for that purpose it was desirable to know approximately what the exposure had been. Similarly as regards a hunting picture taken with an apparently abnormally long exposure, everything depended upon the point of view relatively to the motion of the animals; under

unfavourable conditions in this respect a sharp result could sometimes be obtained with relatively long exposures by a suitable movement of the camera during exposure. In any case, the first step towards scientific accuracy in photography was to have the means of knowing the actual duration of the exposure given in any particular case, and he thought they all felt that Mr. Hunter's apparatus would be of considerable service in that direction.

Mr. Horace Beck said that there was one question raised by Mr. Rosenhain on which he would like to say a word or two, namely, as to following an object by swinging the camera. He had found this a most satisfactory method, and by doing so he had taken photographs at an actual speed of about 1/100 of a second of tobogganning on some of the ice runs, whereas by keeping the camera steady he required 1/500 of a second to get equal sharpness of the moving object. In this case he had a specially speeded up shutter for the experiment.

Another point which arises is that if one is taking the total length of exposure in the method which Mr. Hunter is using, one takes it from the time the lens begins to uncover, and goes on to the time it is completely closed. In many shutters the rate of opening is a comparatively gradual one at starting, and it then opens more rapidly to a maximum, closing again more rapidly at first, and then more gradually. This applies particularly to any shutter in which a circular aperture is opened by means of a straight-edged plate. Thus, only a very small amount of light enters the camera at the time the shutter is beginning to open and just closing.

Supposing that in a correctly timed exposure the first and last 1/10th of the exposure do not give sufficient light to appreciably affect the plate. It can be calculated that in some shutters which, taken in Mr. Hunter's method, would show a certain speed, in actual practice would give results of 30 per cent. difference as far as movement of the object goes.

Another reason why one wanted correct speeds for photography was that when one was using a slow speed a person who had a good, steady hand, could hold a camera comparatively steady for a tenth of a second, and occasionally for longer, but with the ordinary per-

son it was perfectly certain, in the vast majority of cases, that if he had a shutter which, when set at the nominal 1/25th of a second, was really running at a third of a second, he could not help shaking the picture. Then, with reference to Mr. Hunter's actual instrument for testing, he thought it was an extremely ingenious arrangement, particularly the device of the rubber ball between the two cones.

He believed if Mr. Hunter wanted to apply it to greater power he could do so by putting a solid bicycle tyre in between, which would hang loosely on one cone. Then Mr. Purser raised another point, namely, the difference that occurred either by accident or malicious intent, owing to foreign matters getting into the shutters. Many of the shutters his firm had received for repairs were in such a condition that they reminded him of the "Mad Hatter's" watch in "Alice in Wonderland," for, when he complained of the treatment it had received, the March Hare replied, "but it was the best butter."

Mr. Chalmers said he was very much interested in Mr. Hunter's paper. As an easy means of measuring the speed of the disc and determining its constancy, the method described by Dr. Drysdale, in his paper on "Stroboscopy," read to the Optical Society some time back, might be useful. Mr. Hunter had encountered difficulties due to the necessity of setting on the two ends of a scale; a similar difficulty occurs in the "ophthalmometer," and is there obviated by the device of "doubling the image." This device would render the method independent of the constancy of the shutter, and thus enable it to be applied to test this constancy.

He thought that the ideal method of testing shutters would be one which gave an actual picture of the shutter at regular intervals during exposure. He, therefore, suggested the use of a photographic recording drum, on which images of the shutter were focused by the aid of a good lens. If the shutter were illuminated by intermittent light, pictures of the shutter at regular intervals could be obtained. As a preliminary test of the method he proposed using an alternate current arc with a period of 1/100th second.

He anticipated no serious difficulties for low speeds, but expected that it would be necessary to use a tuning

fork to obtain the required frequency for high speed shutters. This method should give the efficiency and time of the shutter for any aperture, and should be specially applicable to roller blind and focal plane shutters. The suggestion of Mr. Beck that not only the actual time of exposure, but also its value for use with exposure tables, should be given, was most valuable.

Mr. H. S. Ryland congratulated Mr. Hunter on his method of attacking a difficult problem. The chief source of inaccuracies appeared to him to be that the speed of the disc was not constant. This could be eliminated by a self-recording centrifugal speed indicator. Then the speed could be easily read at any moment, or a key could be arranged, so that when depressed (as it would be at the instant of taking a reading) it would mark the speed indicator band.

As to the need for a means of marking shutter speeds, that undoubtedly existed; but it was annoying when one had a shutter and could not tell the difference between 1 second and 1/50th of a second. With such delicate mechanism as they had in shutters, it would be almost impossible to keep the speeds constant, but one did expect the ratios between the various marked speeds to be constant. The shutter should, he thought, be graduated for total time it is open, and its efficiency given at the aperture for which it was marked.

The President observed that except in gathering up the threads of the discussion he did not think he had anything to say before Mr. Hunter replied to the specific points raised. There was one thing borne in on his mind, namely, that in this matter they had one which should be considered by the Optical Standards Committee, as they had ample scope. He thought, however, that they had an agreement at the moment by which they had arranged that they would not interfere with photographic standards. Whoever undertook the work would find there were a great many controversial points, through all the intermediate stages, until they could decide on a standard nomenclature, and the degree of accuracy with which the numerical interpretation could be obtained and recorded.

Mr. Hunter's Reply.

Mr. Hunter, in reply to the various questions, said, as to what was the proper time to engrave on a shutter,

it seemed to him there was little doubt that it was the time for which an image of outside objects was formed by a lens, when the shutter was put close up to the lens, and released. One time which was of importance was the extreme time for which the photographic plate was exposed to light, which was the time during which an image was formed. To know the exposure, of course, it was necessary also to find the efficiency of the shutter. But what he called the time of the shutter gave correct information as to how far the position of a moving object would change during the time the shutter was open.

Of course, before knowing the performance of a shutter it was necessary to find the efficiency. From readings taken with apparatus described it was possible to plot an efficiency curve which gave the state of the shutter at any instant between its opening and closing. This curve was virtually the same as those given by Sir William Abney's method, and the method of finding points on the curve was applicable to all forms of shutter.

With regard to Mr. Conrad Beck's remarks, he (Mr. Hunter) had seen Mr. Kershaw's patent. The apparatus described in it seemed so different from his (Mr. Hunter's) own that he had made no reference to it. As far as he remembered, in this arrangement the shutter was held close to the eye, and a light was observed through the shutter and the radial slots of a disc. No image was formed or used as in the arrangement described that evening. As previously pointed out, the method was applicable only to periods shorter than that of persistence of impression on the retina. His (Mr. Hunter's) method and apparatus may be used in conjunction with a camera to make a photographic test. If it was considered that the visual method was in need of checking this could be done. A visual method, however, was considered more suitable on two accounts: (1) that it could be more rapidly carried out, and (2) that difficulties of getting sufficiently strong illumination would not occur.

It had been suggested that speeds in shutters were scarcely any use, and that a shutter was bound to vary widely in performance from day to day, owing to wear, or to dirt getting on the moving parts. If this was so it would be better not to mark times on the shutter at all.

It was no use saying a shutter gave a hundredth of a second if it really gave a tenth of a second. He (Mr. Hunter) had heard of the use of a rubber tyre between two cones, mentioned by Mr. Horace Beck, for a speed change mechanism. But to his mind it had not the same advantages as the mechanism with rubber ball between two cones. In this case the normal reaction required to give sufficient friction was automatically supplied, while in the bicycle tyre method it had to be specially applied. Greater accuracy of measuring the speed of the disc of the apparatus might be useful, and for this purpose a stroboscopic method would, doubtless, be very satisfactory; or, possibly, the continuous speed recorder suggested by Mr. Ryland.

THE EVOLUTION OF ARTIFICIAL LIGHTING.

By Dr. C. V. Drysdale.

Read before the Optical Society on December 11th, 1906.

Among the various branches of optical science there is one which appears to have been strangely neglected by Opticians—that of the production and measurement of the light, with the properties of which they are familiar. In practically every other industry four chief factors enter, the production, measurement, distribution, and utilisation of the article with which it is concerned.

Opticians have practically confined themselves to the last two as concerning light; that is, they study its transmission or distribution by means of lenses, prisms and instruments, and its utilisation on the retina of the eye or in affecting a photographic plate. A very few give some attention to its measurement, and none to its generation. It is as if an electrical engineer should give no attention to batteries or dynamo generators, or a mechanical engineer to boilers and engines.

The "leaving out of Hamlet" in this case may be condoned by the fact that a very large amount of him is provided naturally by the sun, moon and stars, but throughout the history of mankind the production of light artificially has become more and more important. In the following paper, therefore, I propose to deal as briefly as possible with the history of artificial lighting, with the scientific basis of light production, and with the many recent developments.

Historical.

The commencement of artificial lighting dates from the first production of fire, and this is long before historic times. From then until about a century ago the only known method of producing light was by combustion. According to Atheneast, who wrote in the 4th century B.C., lamps were then not an ancient invention. The lamps used in Greece, or λυχνος, generally consisted of a terra cotta cup with handle and two apertures, one for filling and the other, opposite the handle, for the wick, but sometimes a number of wick apertures were provided. These lamps seem, however, to have

† Encyc. Brit. Articles on lamps.

been used rather for religious purposes than for general artificial lighting, and we find that the ancient Persians, Medeans, Assyrians, and Egyptians, had in the temples of Memphis, Thebes, Babylon, Susa, and Nineveh, ceremonies during which the streets were illuminated at short intervals by bronze or stone lamps, each containing about 100 lbs. of liquid fat with wicks about 3 ins. diameter‡.

Pliny and Livy mention the use of candles, and in the 4th century A.D., under Constantine, lamps and candles were fairly common. From thence onward the use of candles appears to have died out, and they were not again employed until the 12th century, when they were used for the Roman Catholic Mass. After the Reformation, they began to be used in the castles of the nobles. Sperm candles first made their appearance in the 18th century, while paraffin-candles were devised about 1850.

The principal defect in the old candles was the guttering of the wicks, but in 1826 De Milly found that impregnating them with boric acid, the fusion of this material gave stability to the wick until it was entirely burnt away. At the present time candles are generally made from spermaceti, combined with a certain amount of stearic acid; from Palmitine, a wax derived from palm oil, or in Germany and Austria from ozokerit or cérésine wax.

Gas.—Although the so-called "eternal fires" of Baku on the Caspian Sea have existed for ages, and are derived from natural gases, there was no use of gas for lighting purposes until 1792, when Robert Murdoch first produced artificial coal gas. The works of Messrs. Boulton and Watt, in Soho, were lit by gas in 1798, the Lyceum Theatre in 1803, while 1810 saw the inauguration of the Gas Light and Coke Company, and 1813 the gas lighting of Westminster Bridge. From that time the use of gas became very extensive, but until recently the light has been derived from incandescent particles of carbon in the flame itself. This was very uneconomical for two reasons.

Lighting Power of Gas.

In the first place illuminaitng gas, which is derived from the dry or destructive distillation of coal, is much

‡ C. R. Bohm. 'Das Gasglühlicht.'

more costly to produce than water gas, which is produced by the combustion of anthracite or coke in steam, and which has a high heating, but practically no lighting power.

Secondly, the luminous efficiency of the ordinary gas flame is low, partly owing to the fact that carbon at a certain temperature gives off a larger proportion of useless long waved radiations than other substances, and partly because the carbon particles are probably oxydised before their most efficient temperature is attained. If, therefore, the cheaper non-illuminating water gas could be employed to heat a refractory substance having a tendency to selective emission in the luminous portion of the spectrum, an immense gain should be effected.

The oxyhydrogen, or limelight, was in a sense the first step in this direction, and was devised in 1826 by Thomas Hammond, an English officer. Zircon oxide was used by Caron and Linnemann, in 1868. The first application, however, to the non-luminous gas flame seems to have been made by Alexander Cruickshanks, in 1839, who employed quartz or platinum bodies in such a flame, and who actually made a platinum mantle. In 1882 Victor Popp employed a platinum wire net or hat, and platinum and iron wire mantles were used by Jackson, in America, in 1881. Next, in 1883, Fahnelijelm, in Stockholm, introduced rows of needles on thin rods in the form of a comb, of kaolin, quartz, zirconia, lime, or magnesia, into the gas flame.

The final great step in improving the economy of the gas burner was made by Dr. Auer von Welsbach, in 1886, and announced in the German 'Pharmaceutical Post.' He employed an improved Bunsen burner with platinum wire mantle covered with oxides or salts of lanthanium, zirconium yttrium, magnesium or erbium. Since this time the incandescent gas has made immense progress. The mantle principally employed recently has been, according to Professor Fery, a mixture of 98.7 per cent. of thoria with 1.3 per cent. of ceria. The latter plays an important part in the light production, as being dark in colour it radiates more powerfully. Up to a certain point, therefore, it considerably increases the luminosity of the mantle, but if this point is passed the light again diminishes, owing to the cooling of the flame caused by this increased radiation.

It is possible that changes in the quantity of ceria present in the mantle with time account for the great falling off in brightness, which soon occurs with these mantles. There is no doubt, however, that incandescent gas lighting is very low in cost, and it would be more so if gas companies were not compelled to supply illuminating rather than non-illuminating gas.

ARC LAMP.—In 1802, almost immediately after the discovery of the galvanic battery by Volta, Sir Humphry Davy and others experimented upon the spark produced when the current was passed between pencils of different substances. The smallness of the amount of energy available from batteries at first only permitted the formation of intermittent arcs, but in 1809 Sir Humphry Davy showed a continuous arc about three inches long at the Royal Institution. He noticed that the most vivid light was given by carbon pencils, and these have been employed ever since. The first practical use of the arc light was in Paris, in 1878, and with the improvement of the dynamo machine its use has rapidly extended.

Sir Humphry Davy's Discovery.

There is little need to go further into this matter here, as most of the developments were in the mechanism for automatically regulating the lamp, and the recent developments in improving the light will be referred to later.

GLOW LAMP.—Soon after the discovery of the battery it was found that thin metallic wires could be made red or white hot, or even melted, by the passage of the current. This seems also to have been first due to Sir H. Davy, who, in 1810, passed the current from his 2,000 cell battery through a platinum wire one-thirtieth of an inch in diameter and 18 inches long, and stated that this wire instantly became red hot, afterwards becoming white hot and giving off a light of unsupportable brilliancy*.

The incandescence of platinum was afterwards employed in actual incandescent lamps by Edison, who found that by frequent heating and cooling, thus driving out the occluded gases, the platinum would stand a much

* "Electric Lighting by Incandescence," W. T. Hammer, 'American Electrical Review,' March 9, 1907.

higher temperature and give a considerable amount of light. The use of iridium, which has recently come to the front again, seems to have originated with Petrie, and was taken up by Edison and others. The only substance, however, which appeared to combine a highly refractory power, with a suitable resistance, was carbon, which, of course, had to be heated in a vacuous, or, at least, oxygen free space.

The suggestion of using carbon in this way appears to have been first due to Professor Jombard, in 1838, but it was reserved for Edison, in 1879, and Swan to develop it into commercial form. For many years the carbon filament glow lamp was exclusively used, but recently there has been a disposition to revert to metallic filaments, and the more recent improvements will be referred to later.

Having now given a rough idea of the progress of artificial lighting up to a few years ago, it will be well to pause and to consider for a moment the intrinsic nature of the problem. Only by so doing can we realise the recent improvements and see what still remains to be done. It is now universally agreed that light consists of a vibratory disturbance of the ether, which is generally caused by a body whose particles are in a state of rapid agitation. Only such vibrations, however, are termed light which will stimulate the sensation of vision, and it has been found by Angström that these lie between 392 and 757 billions of vibrations per second, corresponding to wave lengths in the ether of from 760 to 390 $\mu\mu$.

Since we know in the case of sound that doubling the number of vibrations raises the pitch by an octave, we can say that the whole range of visible vibrations are within an octave. The successive "notes" of this octave are represented by different colours, as in the following table, taken from Barker's Physics:—

Colour	No. of Vibrations billions per sec.	Wave length $\mu\mu$.
Ultra Red	... 370 ...	810.0
Red	... 428 ...	700.0
Orange Red ...	... 483 ...	620.8
Orange	... 502 ...	597.2
Orange Yellow ...	... 510 ...	587.9
Yellow	... 516 ...	580.8

Colour	No. of Vibrations billions per sec.	Wave length $\mu\mu$.
Green	... 569 ...	527.1
Blue Green ...	... 590 ...	508.2
Cyan Blue ...	... 604 ...	496.0
Blue	... 634 ...	473.2
Violet Blue ...	... 684 ...	438.3
Violet	... 739 ...	405.9
Ultra Violet ...	... 833 ...	366.0

Vibrations in the Spectrum.

Although these are the limits of the vibrations which affect our vision, it is well known that this is an extremely small part of the total range of possible vibrations of a body. Vibrations from 30 to about 40,000 per second are recognisable as sound, from that up to 400 billions as heat, while above 800 billions they exist as ultra violet light. The lowest number of vibrations detected in the spectrum is 10 billions or 30,000 $\mu\mu$, found by Langley with his bolometer, while Cornu has measured them up to 1,622 billions, or 185 $\mu\mu$.* An incandescent body gives out an immense range of these vibrations extending probably from the lowest frequency up to a number which is higher the greater the degree of incandescence.

It will be easily seen what the real problem of economical artificial lighting is. We require to stimulate a substance to vibrate in such a manner as to give out vibrations within the limits of 392 and 757 billions per second, and none above or below these limits. All vibrational disturbances are communicated to the ether, and imply radiation of energy, so that as far as lighting is concerned all vibrations outside the visible spectrum are useless and wasteful.

On the other hand, it would hardly be satisfactory, even if possible, to produce a vibration of one frequency. This would mean a light of one colour only, and although the efficiency might be perfect, it would give most extraordinary effects with coloured objects. The ideal light must, therefore, have a number of rates of vibration, distributed over the whole range of the spectrum, and of

* Askinass gives 60-70 μ as the greatest, and Schuman 125 $\mu\mu$ as the smallest wave-lengths experimentally determined.

such amounts as to give, approximately, the same appearance to the eye and the same colours as daylight.

To realise more exactly the nature of light we must consider for a moment the nature of the vibration in the ether. It is somewhat difficult to obtain a clear picture of what is going on in this case, but it may perhaps best be understood by a simple analogy. If we take an electrically-charged body, we all know that what are called lines of force, or electric strain, exist in the ether round it, and these lines are in the form of curves starting out from the charged body.

We may, therefore, think of the body as having a number of elastic threads attached to it. Now, suppose that the charged body receives a vibrating motion in a direction at right angles to the line of force considered. The result will be the same as when the end of a string is oscillated. Pulses or waves will travel along the cord or line of force, and these are the transverse electric waves which, when sufficiently rapid, constitute light. As a matter of fact these electric pulses produce magnetic ones at right angles to them, so that we have what are called electro-magnetic waves, such as are always produced by alternating electric currents in conductors, and which are used in wireless telegraphy; the waves thus produced being too slow to affect the eye.

In order to produce these waves of light we have, therefore, two courses open to us. We may either attempt to produce these electro-magnetic vibrations in the ether directly by alternating currents with frequencies of 400 to 800 billions per second, or we may excite electrical and magnetic vibrations in the particles of a substance which impart them to the ether. The first is the more scientific procedure, but it has hitherto shown no prospect of success.

The Cause of Light.

The most rapid electro-magnetic oscillations yet produced have been of the order of one-eighth of a billion per second, instead of 400 billion, which is the minimum necessary for light. These vibrations are produced by causing sparks to pass between little spheres, thus causing oscillations within them, but it can be shown mathematically that the spheres would have to be reduced to about the size of atoms to produce oscillations of sufficient frequency. In fact, the light emitted by any substance

is undoubtedly due to electro-magnetic vibrations in its molecules.

The only other course open to us, therefore, is to stimulate the vibrations by agitating the molecules of a substance, and this can be effected either by heating the substance, by passing an electric current through it, by vibrations of other rates, or by causing a shower of particles at a very high velocity to impinge upon it. Modern investigation has proved almost beyond all doubt that instead of substances being composed of indivisible atoms each of these atoms is composed of 700 or more smaller particles, called electrons. The atoms, in fact, may be considered as aggregations of electrons, like mulberries, or, rather, like swarms of bees, each swarm keeping in one body, although they may move relatively to one another.

These electrons really revolve round their common centre of inertia like planets round the sun, and whenever such groups collide their internal motions may be accelerated. It has long been known that the heating of a body consists of imparting a vibratory motion to it, and thus making its particles move at a higher speed, and collide with each other more frequently; and if these collisions are sufficiently rapid and violent the motion of the electrons in the atoms will be rapid enough to produce light waves in the ether.

Principle of the Arc Lamp.

Whenever an electric current passes through a substance it implies that a stream of electrons travels from the negative to the positive pole. Each atom is supposed to have one or more electrons (six being the maximum) which can be detached, and in the case of a conducting body these detachable electrons are continually handed on from one atom to the next, thus constituting the current. These interchanges are naturally accompanied with rapid motion, manifested as heat, or as light, if the current is very strong. This is the principle of the electric glow lamp, or arc lamp.

Thirdly, we have a method of stimulating certain vibrations in a body by means of other vibrations of a different rate. This is illustrated in the two phenomena of calorescence and fluorescence. Each compound substance being made up of molecules composed of various numbers of various sorts of atoms, will have

a definite time or times of vibration which it will most easily respond to, and it may even vibrate at these rates when stimulated by other vibrations of quite different rates. As an example, a pier in the sea, when struck by the waves at intervals of a few seconds, may tremble at a very high rate.

This is akin to the phenomenon of calorescence, where some bodies, when heated to a comparatively low temperature, will give out a light of quite a high frequency. In fluorescence we have almost the same effect, except that visible light is sometimes emitted by a substance, when illuminated by a light of higher frequency, such as that we term ultra-violet light. In phosphorescence the result persists for some time after the exciting cause is removed.

Lastly, we may provoke the natural vibrations of the body by impacting upon it a shower of particles at an extremely high velocity. This was first done on any considerable scale by Sir W. Crookes, who found that when a tube was exhausted to a high degree, and a discharge was passed through it, substances placed opposite the negative pole or cathode became intensely luminous. He came to the conclusion that this was due to a stream of what he termed "radiant matter," which was projected with high velocity from the negative pole, and stimulated the vibrations of the substance on which they impinged.

It is now supposed that this radiant matter consists of electrons projected off the negative pole with a velocity approaching that of light, and that in consequence of the small number of air molecules present, these are able to traverse long distances. This method of producing light by the impact of showers of electrons is termed "luminescence," and it has been greatly studied on the continent by Wiedemann, Plucker, Lenard, Hittorf, and many others. The recent researches on the radio-active bodies, radium, polonium, etc., have shown that these bodies are continually projecting electrons which will cause luminescence in suitable substances.

With the foregoing in mind we may now pass to the recent remarkable developments in artificial lighting.

Incandescence Methods.

From what has been said as to the producing of vibrations by heating it is fairly obvious that little economy

can be expected from this method under ordinary circumstances. The blind agitation or joggling of the particles produces vibrations, it is true, but (in the case of most solid bodies) of every sort mixed together. Below a temperature of about 400° C., although a considerable amount of energy is radiated, it is below the limits of vision, and as the temperature is raised and visible radiation is produced, there is yet a great increase of the invisible radiation.

It was first stated by Stefan, and it has been since confirmed by the researches of Boltzmann, Lummer, and Pringsheim, Rosa, and Féry, that the total radiation is proportional to the fourth power of the absolute temperature. For an absolutely black body Kurlbaum has stated that the radiation is .01763 calories per square cm. from a surface at 100° C. to one at 0° C.* We have also the important law of Wien, which states that the dominant wave-length, or wave-length at which the greatest energy is radiated, is inversely proportional to the absolute temperature. Calculations which the writer has made from the results of Paschen and Wanner give for the dominant wave length in 1,000ths of a millimeter $\lambda = \frac{2888}{T}$. Hence, for a wave-length of .54 μ, which is approximately the most luminous part of the spectrum $T = \frac{2888}{.54} =$ 5,350° C. absolute, or 5,075° C. on the ordinary scale, which is given by Féry as about the temperature of the sun.

From these considerations it is easily seen that almost the only possibility of obtaining a reasonably efficient source of light by incandescence is to have a very high temperature. The following table, from Dr. Fleming's "Electric Lamps and Electric Lighting," shows the approximate temperature corresponding to various degrees of incandescence.

	Centigrade.
Bodies just visible in dark	400
Dull red heat at	700
Dull cherry-red	800
Full cherry-red	900

* Dr. C. Féry, 'Rayonnement Calorifique et Lumineux,' p. 69, gives $4.74 \times 10^{-12} T^4$ as the radiation in watts per square cm. from a carbon filament.

	Centigrade.
Melting point of silver	945
Clear red heat	1,000
Melting point of gold	1,045
White cast iron melts	1,040
Orange red heat	1,100
Bright orange	1,200
White heat	1,300
Steel melts at	1,300
Bright white heat	1,400
Dazzling white heat	1,500
Palladium melts at	1,500
Wrought iron melts	1,600
Platinum melts	1,775
Iridium melts	1,950

Experiments have been made by Langley, by Merritt and Marks, and by Nicholls, at the Cornell University, upon the proportion of light to total energy in various sources.

Source of Light.	Watts per candle.	Ratio of Luminous to total energy.	Effective Watts per c.p.
Candle	86	2 to 3%	1.7 to 2.6
Oil lamp ...	57	3%	2.7
Petroleum lamp	42.8	3%	1.3
Argand gas lamp	68.8	2.4% 4% (Langley.)	1.65 to 2.8
Electric glow la'p	3½	3 to 7%	.105 to .245
Electric arc ...	.8	5 to 15%	.04 to .12
Magnesium wire		15% (Fleming) 35% (Rogers)	
Electric discharge in gases ...		33%	
Sun (Langley) ...		35%	
Mercury vapour lamp	.52	60%	.34
Firefly (Langley)		nearly 100%	

The Welsbach Lamp.

As far as flame incandescent lamps are concerned, it is evident that the chief requisite is a highly refractory substance which will stand an extremely high temperature without rapid deterioration, and a flame of high

temperature. This is what has been done in the case of the oxyhydrogen light, and in the modern Welsbach lamp, in which a thoria mantle is used. It is not sufficient, however, that a large proportion of the radiation should be luminous, we must also have a good emissivity. It has been shown by Swinburne, as before stated, that a certain proportion (about ½ per cent.) of cerium oxide, which is dark in colour, is necessary in order that the radiation is sufficient. Above this amount the efficiency is diminished, and this is attributed by Swinburne to the increase of radiation actually lowering the temperature of the mantle.

In electric glow lamps the refractory nature of carbon, combined with its conductivity, has made it appear, until lately, the only possible material, and research was almost stopped in that direction for many years. It has been found impossible to raise the efficiency of the ordinary carbon filament much above three watts per c.p. owing to the throwing off of carbon particles and the blackening of the bulbs*.

According to Dr. Fleming,† Mr. Meritt, of Cornell, found the following for the luminous efficiency of a 16 c.p. Edison glow lamp at different voltages:—

Working volts.	Candle power.	Power absorbed in Watts.	Watts per candle power.	Luminous radiation in Watts.	Luminous efficiency %	Luminous radiation Watts per candle.
74.2	.9	34.6	38.0	.18	.5	.20
91.6	4.8	56.2	12.0	.68	1.2	.142
97.3	7.3	64.6	9.0	1.13	1.7	.155
100.3	8.9	69.3	7.8	1.62	2.3	.180
107.6	14.6	81.6	5.6	2.97	3.6	.204
109.3	16.3	84.4	5.2	4.57	5.4	.280
124.1	38.2	115.4	3.0	7.46	6.5	.195

* The G.E. Co. of America has recently made filaments of an efficiency of 2½ watts per c.p. by taking an ordinary treated filament and heating it to 3,000 or 3,700° C. in an electric furnace.

† 'Electric Lamps and Electric Lighting,' J. A. Fleming, F.R.S., p. 83. The figures in the last column have been calculated by the present writer.

In an American publication, 'The Cyclopædia of Applied Electricity,' the following is given as the temperature of a glow lamp filament at different efficiencies.

Glow Lamp Temperatures.

Temperature.	Watts per candle.	Temperature.	Watts per candle.
1200	9.8	1275	4.5
1225	7.5	1300	3.5
1250	5.75	1325	2.8

The variation of efficiency and life of these lamps is given by the same authority as follows: —

Voltage % normal.	Candle power % of normal.	Watts per c.p.	Life % normal.
90	53	5.36	—
95	78	4.26	310
100	100	3.5	100
105	129	2.95	39
110	169	2.53	16

These figures show both the extreme importance of correct voltage for carbon filament lamps and the hopelessness of attempting to secure a much higher efficiency from them, without radical change in their construction.

The large amount of research on luminous radiation on the continent has, however, led to the attempt to gain higher temperatures with the most remarkable results.

The first of these was obtained by Dr. Nernst*, in 1899, who, considering that it would be impossible to

* "Nernst Electric Lamp," J. Swinburne, 'Soc. Arts Journal,' 47, pp. 253-260, 1899. E. de Fodor ('Zeitschrift Electrolech Wien,' 17, pp. 172-178, 1899) states that Jablochkoff, in 1877, patented a lamp consisting of a strip of kaolin 38 by 10 by 2 mm., and Edison did the same with other refractory substances, and used a heating spiral.

obtain metallic filaments of a sufficiently high melting point, tried some of the infusible oxides used in the limelight and gas mantles, and found that they were conductors at high temperatures. He adopted a mixture of zirconia and yttria, in the form of a small rod, which is electrically heated at starting by a small coil, which is automatically switched out when the filament begins to conduct. These lamps seem to average about two watts per candle with a life of 500 hours. Messrs. Drake and Gorham have kindly lent me some of the most recent of these lamps.

It has now been found that a number of metals will serve as filaments for these high temperatures. Dr. Auer von Welsbach was the first to use osmium, and these lamps have been on the market for some time. They are only made, however, up to about 25 volts, requiring four in series on 100 volt circuit. Next, Bolton, at the works of Messrs. Siemens and Halske, produced the Tantalum lamp, which is coming into extensive use. Tantalum appears to have a melting point of about 2,250° C., and can be drawn into wire. It makes a very long and delicate filament, but seems to work well on direct currents up to 110 volts, with an energy consumption of about 1.5 watts per candle, and a useful life of 500 hours. With alternating currents, however, the lamps blacken and the filament soon breaks.

A Remarkable Achievement.

A number of other lamps have been produced with iridium, tungsten, molybdenum and zirconium filaments. The Osram lamp consisting of osmium and tungsten has been found to have a consumption of about 1.08 watts per candle *after* running 1,000 hours. This is a most remarkable achievement, and means that the consumption of energy is only one-third or one-fourth of that in the ordinary carbon filament lamps. Incidentally it should be mentioned that these lamps are much less sensitive to fluctuations in voltage, and this should make them good secondary standards for photometric work.

A most interesting point in the construction of these lamps is that many of the filaments are made from colloidal solutions or suspensions of the metals, the state of which is rendered visible by the ultra microscope. Many of you will remember the exhibition of this instrument

by Messrs. Siedentopf and Zigmondy, in 1904. Messrs. Reichert, of Vienna, have just introduced a simplified form of this device, and Mr. J. H. Baugh has kindly arranged to show you a colloidal solution of silver with it this evening.

Attempts have been recently made to improve the efficiency of the arc lamp. This has been effected in the case of the flame arc lamps, in which calcium and magnesium salts are introduced, which give in some cases two or three times or more light for the same energy consumption. I have some doubt whether this is due to increase of temperature or whether it is not more probably a form of calorescence or fluorescence. On examining the spectrum of such an arc we generally find a number of very bright lines indicating that there is a well marked selective emission bringing a much larger proportion of the radiation within the spectrum. The combination of the use of these materials with improved disposition of the carbons enables one to get a mean hemispherical candle-power of about 2,000, or more, for an expenditure of 500 watts, or more than 4 c.p. per watt.

As to luminescence lamps the only practicable one yet devised is the mercury vapour lamp of Cooper-Hewitt, Bastian, and others. The luminous efficiency of this lamp is said to be as high as 60 per cent., but it is somewhat difficult to reconcile this with the energy consumption which has been stated from tests made at the Reichsanstalt to be .52 watts per c.p. without, and .88 watts per c.p. with, the necessary resistance, thus making it appear little more efficient than the more recent incandescent lamps. The fact is that experimental tests of the luminous efficiency are very liable to be deceptive, and much more work is needed in this direction. The great difficulty with the mercury vapour lamp, however, has been with its extraordinary colour distorting effects, and many attempts have been made to introduce fresh lines into its spectrum. The only devices, however, which have been at all successful, are those of employing cadmium or by the fluorescence of rhodamine.

Many minerals fluoresce somewhat brilliantly in a cathode stream, chief among these being corundum, or silicon carbide, barium platinocyanide, scheelite, or calcium tungstate, willemite, dolomite, and zinc blende. None of them, however, appear yet to be within practi-

cal politics, not so much on account of efficiency or colour, as on account of the costly nature of the cathode ray-form of energy. This difficulty should not be unsurmountable when the importance of the subject is realised. The recent work of Paulsen in the production of undamped electrical oscillations of high frequency opens up new possibilities already.

Finally, I should like to plead for a greater interest in this important subject in this country, both by Opticians and by others. Although much has been done in the past, much yet remains to be done for human beings to catch up the firefly. Surely there is more human and commercial advantage to be gained from conquering these problems, than in wrangling over optical politics. It is time that we united in an effort to show other nations that instead of quarrelling among ourselves while they do the work and reap the advantage, we are doing something worthy of the country we live in.

Mr. Rosenberg said that progress was also being made in gas-lighting. Experiments were being made with organo metallic compounds such as magnesium, calcium, or alminium, in the form of an ethyl, ethylate, or methylate, or similar silicate compound; the gas was passed over these volatile compounds, and on ignition gave the same effect as the ordinary mantle, and the light-giving power did not decrease with use.

Similar methods had also been used to improve the candle—the organo-metallic compounds being mixed with the fats or paraffin.

Mr. Leon Gaster said that the tantalum lamps had stood the efficiency and life tests on direct current, but were not satisfactory on alternating current, more particularly with the higher frequencies.

The "Osram" lamp and the Zircon "Z" lamp worked satisfactorily on both circuits, and efficiencies of 1 watt per candle were claimed.

The lecturer having referred to the Cooper-Hewitt mercury vapour lamp, he would like to mention also another vapour lamp, which was Macfarland Moore's invention. This vacuum lamp worked only with alternating current at high voltages and at very high frequencies, and consisted of a glass tube of convenient diameter, and of any desired length, having electrical conductors, her-

metically sealed into the opposite ends, and the air within exhausted to such a degree as to bring it to the point of conductivity for electric current of available pressure. The passage of the current raised the rarefied gases to a state of incandescence, and the lighting so produced commended itself on account of the low intrinsic brilliancy and consequent avoidance of retinal fatigue, the diffusion obtainable being far more perfect than with light sources of a point character, the efficiency of the system was claimed to be very high. He was only mentioning this method in order to indicate the direction of researches worthy to be more closely followed in the future.

Although the paper dealt exhaustively with the evolution of artificial illumination, he ventured to think that the question of the influence of artificial light upon sight would have, in the future, to receive a greater attention than it had in the past. The physiological factors in illumination and photometry ought to be investigated from more than one point of view, and for that purpose the Optical Society seemed to be best suited to have such work undertaken by its members. He took that opportunity to suggest that oculists, when examining their patients, should also try to keep a record regarding the nature of the light used by their patients; valuable results might be obtained from comparing such data, and perhaps a more corroborative conclusion could be drawn showing the relation between the strength and colour of the illuminants, and the diseases of the eyes.

The question as to what was really the best and most suitable method for artificial illumination, and the determination of the minimum amount of light necessary for different purposes, would be very useful. He should like to mention that at a recent meeting of the "Illuminating Engineering Society" of the United States, a discussion, in which a great number of oculists took part, took place on the physiological aspects of the subject, but the opinions expressed did not seem to be quite corroborative, proving that the subject had not as yet been sufficiently investigated. The introduction of many new lamps and improved methods of illumination was only of recent date, and, therefore, the subject would have to be more closely studied now. Anything the Optical Society could do in that connection he was sure

would be much appreciated by all concerned with the use of artificial illumination.

Mr. V. H. Mackinney said that he had made a series of life tests on lamps, including 12 Osmium and 12 Tantalum lamps; the results showed an average life of well over 1,000 hours, the efficiency was 1.2 watts per candle at the beginning, and 2 watts after 1,000 hours for the Osmium, while the Tantalum lamps were rather less efficient after 1,000 hours owing to blackening.

The Osmium lamp showed no blackening if it were not run above its *nominal* voltage, while the Tantalum gave a black band just in front of the filament. This uneven blackening necessitated a modification of the mean spherical reduction factor for different times in the life. This difficulty could be avoided by the use of an "Ulbricht sphere," but on Mr. Howe's suggestion the sphere was replaced by a one meter cube and the results were quite satisfactory. He considered that the difficulties of obtaining accurate comparisons of the different coloured lights had been exaggerated, and claimed an accuracy of 2 to 3% for his own observations.

Mr. Chalmers thought that as a compromise between the line spectrum and the continuous spectrum, it might be possible to form a spectrum consisting of a number of *wide* bands; this, though probably not so efficient as a line spectrum, might be, physiologically and practically, more desirable.

Dr. Drysdale, in reply, said that the use of refractory materials in enriching gas was most interesting, though the loss of the refractory materials limited the application. The physiological questions raised by Mr. Gaster were of the utmost importance, and would require careful investigation. The ideal light of the future would, he thought, consist of a number of bands in the visible spectrum.

THERMOMETER GLASS.

By J. A. Harker, D.Sc.,

First Assistant in Physics Department, National Physical Laboratory.

Read before the Optical Society, January 16th, 1907.

ABSTRACT.

The lecture began with a resumé of the fundamental principles of thermometry, particularly dealing with those types of thermometers in which the dilatation of a fluid is employed as the measuring principle.

The construction and use of the standard gas thermometers at the International Bureau of Weights and Measures, at Sèvres, and at the National Physical Laboratory were illustrated by numerous slides.

The fundamental scale of temperature, that of the hydrogen thermometer, was defined, and its limitations, both for high and for very low temperatures, discussed. At high temperatures it is impossible to find a substance for the reservoir which is at the same time not attacked by hydrogen and does not permit diffusion through its walls. Hence, other gases such as nitrogen are used in the place of hydrogen at high temperatures. In all but the most accurate work the nitrogen-scale may be considered identical with that of hydrogen. The gas-thermometer is, however, complicated, difficult to use and is quite unsuitable as a general working standard. Therefore it is of fundamental importance to have some more generally available instrument.

The construction and methods of standardisation of standard mercurial thermometers were then dealt with, and an account given of the methods adopted to ascertain the relation of the "scale" of these to that of the hydrogen thermometer. Slides were shown illustrating

the methods of comparison in a water bath of such standard thermometers made of "verre dur" with the gas-thermometers at Sèvres. The construction and annealing of high-range mercurial thermometers were then explained, and the defects of the ordinary type of instrument pointed out. The recent improvements in the quality of glass suitable for high-range thermometers were described, and the methods of construction and graduation of the very high-range type of thermometer containing, over the mercury, nitrogen, or carbonic acid under pressure, illustrated.

Tables were shown giving for various temperatures the change of zero and departure from hydrogen of the natural scales of thermometers made of "verre dur," Jena glass 16^{III}, Jena glass 59^{III}, and "Kew standard" glass.

Recent investigation had shown that while the newer glasses possessed substantial advantages at high temperatures over the old Kew glass, particularly as regards zero measurements, the latter was found to depart much less from the hydrogen scale than the rest. Very conveniently a temperature of, say, 62° F., on the *Kew* scale was found to coincide extremely closely with the same temperature as the hydrogen scale, whereas a thermometer made of almost any other glass always read distinctly higher.

For the standardisation and testing of high-range mercurial thermometers, special baths had been designed by the National Physical Laboratory; for work up to 220° C. oil is used, and above that temperature a mixture of fused salts.

As working standards for all work above 100° C. the platinum resistance thermometer is used. Its construction and working were explained and illustrated. The Laboratory was engaged in some work on a series of new thermometer glasses made by Messrs. Powell, some of which had given interesting results.

THE OPHTHALMOMETER.

By W. Ettles, M.D., F.R.C.S.

Read before the Optical Society, February 21st, 1907.

The instrument which will engage our attention to-night is of incalculable importance to us as students of optical problems, since it has laid the foundation of modern physiological optics. By means of it we obtain an accurate measurement of the curvature of the principal dioptric surface of the eye, a measurement which, in conjunction with the refractive indices of the media, enables us to calculate the cardinal points of that organ.

May I remind you at the outset that we have to deal with four media, namely, the cornea, the aqueous humour, the lens, and the vitreous humour. As to the cornea, the most accurate observations are those made by Mattiessen, and published by him in the Festschrift, in honour of Helmholtz's 70th anniversary. He gives it as 1.377. The aqueous humour has an index of 1.3365, as determined by Helmholtz and Reich. Incidentally we may note that Brewster found for it 1.3366 in 1835. Considering how this last observer worked it out, the figure is wonderfully exact. Brewster made a hollow prism by "fastening together at an angle two pieces of plate glass with a bit of wax. A drop of fluid is placed in the angle where it will be retained by the force of capillary attraction." When such a fluid prism is placed in a position of minimum deviation for light of the required wave length, we have—

$$\mu = \frac{\text{Sin } \frac{1}{2}(A + d)}{\text{Sin } \frac{1}{2} A}$$

Where A = the angle of the prism, and d = minimum deviation.

The index of the lens has been less exactly determined. I propose to utilise 1.42. In 1801, our immortal

countryman, Thomas Young, estimated the total index as 1.436. The accuracy of his wonderful work is well shown by the fact that although Listing, in Wagner's Handwörterbuch, concluded that the index was 1.455, and Helmholtz in his earlier work used the same index, later observations by Helmholtz caused him to admit that that was too high a figure, so that in his subsequent writings he assumed the index to be 1.4371. Matthiessen found 1.437, and Stadfelt, in 1899, in an article of great merit, found the index to be 1.435.

I should like, "*en passant*," to draw your attention to a method we owe to the genius of Wollaston, whose name will be before us again in connection with his prism, and which method has furnished us with many

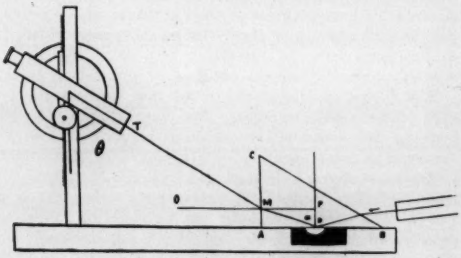


FIG. 1.

important observations on the media of the eye, partly on account of its accuracy, partly because it enables us to deal with fluids or semi-solids in minute quantities such as is the case in ophthalmology, and partly because the medium under examination need not be perfectly transparent, as is again the case with the cornea and lens.

In this procedure, a drop of fluid, or a portion of lens substance is applied to the lower face of a rectangular prism. This prism is placed over a small cavity in the surface supporting it. The drop is then viewed by homogeneous light traversing the hypotenuse, and reflected from the lower face into a telescope, which can be so lowered and inclined that its optic axis is always a

radius of a circle, having the drop as centre. One sees then the drop through the telescope, which is then lowered until the drop vanishes and the light is totally reflected.

The angle α is the critical angle. If we know the index of the prism (μ) and the angle of the telescope with the vertical (θ), the index of the medium (μ^1) is obtained from the formula—

$$\mu^1 = \mu \sin \alpha \text{ and } \mu^1 = \sqrt{\mu^2 - \cos^2 \theta}$$

The transparent vitreous is easily determined, and is found to be 1.3365, *i.e.*, it is identical with the aqueous. This being so, the crystalline lens is to be regarded as being immersed in a homogeneous fluid—the aqueo vitreous—of an index of 1.3365, from which we find its relative index—

$$\mu = \frac{1.42}{1.3365} = 1.06$$

THE CORNEA.

The refractive power of the cornea is a good deal higher than most people imagine, because their attention as regards increase in dioptric power is so much given to the phenomena of accommodation. We shall see, however, that the lens is relatively weak, and that the cornea has more than two and a half times its power.

For easy calculation, let us assume that the radius of curvature is 8 mm., and that it is a single spherical surface separating air ($\mu = 1$) from aqueous, whose index we will take as water (1.33). Then we find that the anterior focal length is—

$$F_1 = \frac{R}{\mu - 1} = \frac{8}{1.33 - 1} = 24 \text{ mm.}$$

Rays emanating from the first focal point in air are parallel in traversing the second medium. Hence, the power of such a surface is represented by a convex lens in air which will bring parallel rays to a focus at a distance of 24 mm. That is why we say its dioptric power is the inverse of F_1 and not of F_2.

$$F_2 = \frac{\mu R}{\mu - 1} = \frac{1.33 \times 8}{1.33 - 1} = 32.$$

$$\text{D. then} = \frac{1\,m}{24} = \frac{1000}{24} = 42 \text{ D.}$$

More exactly, taking the index 1.3365 and an average radius 7.8 mm., we find it to be 43.14 D.

The Corneal Substance and its Posterior Surface.

But, it may be said, what about the cornea itself? It has a higher index than the aqueous, and its posterior surface, if not concentric with the anterior, will act as a positive or negative meniscus as the case may be. Bearing in mind our figure 43.14 D. derived by neglecting these two factors, let us see what we get by using them, and what our error really is.

The average radius of the posterior surface is 6.22 mm. This has been found in the living eye by Tscherning, who was the first to measure its value, and also by injecting gelatine into the anterior chamber, removing the cast so formed, and measuring it directly with the ophthalmometer.

Here we have cornea ($\mu' = 1.377$) and aqueous ($\mu'' = 1.3365$).

$$F'_1 = \frac{\mu' R}{\mu'' - \mu'} = \frac{1.377 \times 6.22}{1.3365 - 1.377} = -\frac{8.565}{0.0405}$$

$$\text{And D.} = \frac{1}{F'_1} = \frac{0.0405\,(1000)}{8.565} = \frac{40.5}{8.565} = -4.73 \text{ D.}$$

The power of the anterior surface is—

$$\text{D.} = \frac{1.377 - 1\,(\text{M})}{7.8} = 48.3 \text{ D.}$$

$\therefore$ the resultant power is $(48.3 - 4.73) = 43.57$ D.

By neglecting the cornea and the posterior surface we got 43.14 D., and our error is $(43.57 - 43.14) = 0.43$ D.

How small this is, is well shown when we regard the total power of the eye. Its anterior focal length is 15.5 mm., corresponding to a power of 64.5 D., and our error is then only 0.6 per cent., one we may safely neglect.

The Power of the Lens.

I have said that the cornea is $2\frac{1}{2}$ times the power of the lens. In its resting state, the lens has an anterior radius of 10 mm., and a posterior of 6 mm., with a relative index of 1.06.

$$\text{Since } D = \mu - 1 \left(\frac{1}{R_1} + \frac{1}{R_2} \right)$$

$$\text{We have } \quad .06 \left(\frac{1}{10} + \frac{1}{6} \right) = \frac{1000}{63} \text{ or } 16 \text{ D.}$$

$$\text{Therefore, } \frac{42}{16} = 2\frac{5}{8}$$

We have seen that the cornea is the most powerful of the dioptric surfaces, and that its anterior surface mainly determines the path taken by rays traversing the eye. A moment's thought will prove that this is necessarily so since the greatest change of index occurs here. Hence also, any want of sphericity, in other words, any astigmatism, will be much more in evidence when it affects this surface, than would be the case with the other surfaces. It is this fact which gives to the ophthalmometer its usefulness in estimating the degree and meridian of corneal astigmatism.

A true astigmatic surface, such as a torus, produced by the revolution of a circle about an axis other than its diameter, is only approximately met with in the eye.

The Shape of the Cornea.

It is now nearly 200 years since Petit found that the corneal periphery was flattened, and that the surface was not truly spherical. Thanks to the invention of the disc bearing his name by Placido, in 1880, we can readily study the reflected images under varying angles of incidence.

On looking at the reflex of a normal eye, we perceive the minute but regular circle reflected by the pole. When the observed eye is directed to fix the margins of the disc the images become drawn out into ellipses, prolate or oblate, according to the direction of fixation.

We see, moreover, that they are assymmetrical, especially near the corneo-scleral margin, from which we conclude that the curvature diminishes, and that in an irregular manner.

By means of the Helmholtz ophthalmometer Aubert found that the cornea is to be regarded as a flattened pyramid with a more or less truly spherical summit. And other workers, notably Sulzer, have confirmed this. We thus see that the idea that the cornea is an ellipsoid of revolution about its major axis must be set aside. Its general adoption is due to the influence of Landolt, who assumed it to be such a figure. However, we may discard his conception of it without any diminution in our admiration of his magnificent work in almost every branch of ophthalmology.

If a mathematical figure be desired to express its general curvature, we may regard the section of the cornea as approximating a hyperboloid of revolution on its major axis, but in point of fact it has no axis of symmetry, and on that account the old angle alpha loses its significance.

The following table is the mean of measurements I made on six eyes, using the Javal-Schiötz ophthalmometer, and working on the nasal half of the horizontal meridian. The eyes were all emmetropic.

0° = 43	D. =	7.7	mm.
10° = 43	D. =	7.85	,,
15° = 42.5	D. =	7.9	,,
20° = 38.5	D. =	8.7	,,
25° = 32.5	D. =	10.4	,,
30° = 21.5	D. =	15.2	,,
35° = 21.0	D. =	16.0	,,

The angular aperture of the cornea is about 86°, so that we see that the optical and spherical part would be limited by a cone, whose base is about 30°.

Radius of the Cornea.

The radius of curvature of the polar area varies in different individuals, and it has no direct relation to errors of refraction. Tscherning and Bourgeois found it to be 7.8 (43.1 D.). So far I have measured only 82 normal eyes, and in these the mean is 7.7 (43.83 D.). In the human eye it varies about 1½ mm., but it is exces-

sively rare to find a radius of more than 8.5, or less than 7.0 mm. Within these limits, and in the lower animals, far beyond them, as compare an emmetropic elephant with an emmetropic mouse, there seems to be a constant ratio between the corneal radius and the antero-posterior length of the eyeball. By means of the ophthalmometer, as used in ophthalmic practice, we only measure the differences between corneal curvatures, we cannot use it for the estimation of the distance between the retina and the posterior focal plane of the eye.

Having dealt with the indices, the power, and shape and radius of the cornea, I will now review the evolution of the various methods which have been adopted to discover the shape of the cornea.

Historical Notes.

The first to apply scientific measures appears to have been Jean Louis Petit, in the early part of the eighteenth century. He constructed templates of copper, bearing arcs of known radius, and these he applied to the cornea of a frozen eye, until he found one which seemed to fit accurately. His measurements are not of much value now, but he made the important observation that the cornea was not truly spherical, and that its periphery was flattened.

C. Krause used frozen eyes, cut in halves along the central axis, and these he placed in a dish and laid on the cut surface a micrometer plate. The whole was then immersed in egg albumen and examined with a low power microscope. Owing to the changes which take place in the tension of the eye, the measurements are open to objection, nevertheless Krause's results are but slightly different from those we have now.

The first to observe the living eye was Thomas Young, in 1800. He ascertained the diameter of his own cornea by direct measurement with compasses. The height of the vertex he obtained by viewing his own profile in a system of mirrors and interposing a graduated scale. Knowing the chord and the sagitta he found the radius from the formula—

$$R = \frac{c^2 + h^2}{2h}$$ where c = the semi-chord.

Matters remained in this position until Kohlrausch published in 1839 his method, based on observation of the images reflected by the corneal surface, thus laying the foundation of the modern instrument. His apparatus consisted of a telescope of short focus in the eye-piece plane of which he had two spider threads capable of being approximated by means of a screw.

On either side of the telescope he placed candles, and viewed their reflections. The threads were placed so as to coincide with the images. The observed eye was then removed, and in the focal plane of the telescope he placed a finely divided scale so that the space between the threads could be read directly.

The one objection to this procedure was the liability to error introduced by minute excursions of the observed eye. It is the overcoming of this which is the crowning feature of the modification introduced by Helmholtz in 1854. He applied to the movable eye the principle of the heliometer, by means of which astronomers measured the diameter of the moving heavenly bodies. It consisted, essentially, of a telescope having in front of the objective, a brass box containing two plane glass plates capable of being rotated symmetrically in opposite directions on an axis at right angles to that of the telescope. The plates are so disposed that each covers exactly half of the area of the objective.

When they are transverse to the axis, the object is seen singly, but when inclined, the object is duplicated, and the separation of the images increases directly with the rotation of the plates. The size of the image, or rather, the distance between the images may be empirically obtained by observing a scale and marking on the dial the equivalents of rotation, or it may be calculated, provided that we know the thickness and index of the plates, which are constants, and the angle of incidence of the ray, which is equivalent to the rotation, from the formula—

$$x = 2\,k\,(\sin 1 - r)/\cos r.$$

When x = the separation of the images and k the thickness of the plate.

The drawback to the Helmholtz instrument is the smallness of the reflexes, the distance (1 or 2 meters) at which it has to be used, and above all the number of ob-

servations in various meridians which must be made before useable results can be arrived at.

In 1872 Mandelstamm and Schöler introduced their micro-optometer. It consists of a horizontally placed microscope, in front of the objective of which is a plane glass plate reflecting the image of a luminous screen which can be focused. The method is of considerable service in estimating the radius of the corneal apex.

In 1874, Landolt brought out his *diplomètre*. Instead of the glass plates, he produced a fixed doubling by placing one above the other two weak prisms with their apices in opposite directions. To obtain contact they were made to slide to and fro, and their traverse was graduated so that the size of the image could be directly read. It is interesting to observe that the procedure has been revived by Mr. Inskeep.

In 1880 we have three notable points in evolution. Firstly, the introduction of the Placido disc; secondly, the appearance of a new principle by Blix; and thirdly, the perfecting of the ophthalmometer as a clinical instrument by Javal and Schïotz. Blix utilised twin microscopes, firmly united, and having a common focal plane of the objective. Through one microscope, luminous rays were made to pass so as to produce an image of a figured screen. This was observed through the other microscope. The principle is this. In a convex mirror an image and its object coincide at two points, *a*, at the vertex, and, *b*, at the centre. When the observed eye is in position, the instrument is focused until the image is clearly visible. This marks the corneal vertex. It is then racked forwards until the image is clear again. It now indicates the centre of curvature. The distance traversed denotes directly the length of the radius.

The point aimed at, and so conspicuously attained by Javal and Schïotz, was the provision of an ophthalmometer which would give rapid and reliable indications without any calculation at all. Before I proceed with a description of it,

The Late Emile Javal.

I would here give expression to the deep sorrow with which we learn of Emile Javal's death, which took place on January 20 last, at the age of 67. His services to

ophthalmology in general, and to French ophthalmology in particular, have been of the most brilliant description. Practically all we know of the orthoptic treatment of squint was worked out by his laborious investigations. The place that the ophthalmometer holds as an instrument of rapid precision which has passed from the laboratory into the consulting room is mainly due to him.

It was as the outcome of his translation of Helmholtz's monumental work that the laboratory of physiological optics was established at the Sorbonne, and from which has emanated that stream of splendid work, and able workers, which has placed France in so commanding a position in this department of optical science. His later years were clouded by the most disastrous affliction which can overtake a man—by irremediable blindness. Yet even this he turned into a blessing, for with that keen perception of his opportunities so characteristic of the Jewish race, his skilled analysis of the resources open to his fellow sufferers, as embodied in his work, with the pathetic title "*Entre Aveugles*," is destined to be of the greatest assistance to them. I trust that this Society will not fail to give adequate expression to its feelings at the loss of so great a colleague.

The Cornea as a Mirror.

The human cornea is a brightly polished, transparent, convex surface, and as such it follows the laws governing the formation of images by convex mirrors. If we regard any such surface we see a diminished and erect image of ourselves. It was, no doubt, from their observing this phenomenon which led the ancient Hebrew writers of the Talmud to allude to the pupil of the eye as ISCHON, "little man." The image is a vertical one, and, as can be found by construction, calculation, or actual experiment, using the parallax, it is situated in a plane midway between the vertex and the centre of curvature of the convex surface.

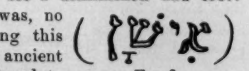


FIG. 2.

The relationship between the object and its image can be readily found by finding the intersection of two of the three following rays emanating from a given point in the object.

a.—A ray parallel to the axis passes through the focus.

b.—A ray directed to the focus is reflected parallel to the axis.

c.—A ray passing to the centre is reflected on itself. Take for example rays *a* and *b* (O = object; I = image; *d* = distance from cornea; R = radius; F = principal focus).

Here we have the similar triangles A B F and H F K.

$$\therefore \frac{AB}{HK} = \frac{BF}{FK} \text{ but } AB = O \text{ and } HK = ab = I$$

$$\text{while } BF = d + F \text{ and } FK = \frac{R}{2}$$

$$\text{So that } \frac{O}{1} = \frac{d+F}{\frac{R}{2}} = \frac{2(d+F)}{R}$$

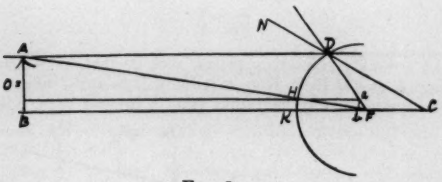


Fig. 3.

And since F is very small as compared to B K, we have—

$$\frac{O}{1} = \frac{2d}{R}$$

$$\text{and } R = \frac{2d1}{O}$$

So, then, the object is the image as twice their distance apart is to the required radius.

If, then, we can measure this distance, the size of the object and the dimensions of its image, we can easily find the radius.

The Principle of Doubling.

So far, the method is that of Kohlrausch, but owing to the unsteadiness of the observed eye, the measurement of the image is difficult and unreliable. This brings us to consider the principle of doubling applied to the instrument by Helmholtz.

Suppose we have the plates fixed at a given angle so as to produce a fixed deviation, or, better still, let us

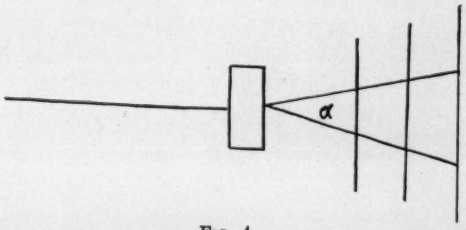


FIG. 4.

take a doubly refracting prism, such as that of Wollaston. A ray of light from a point will be split into two rays symmetrically deviating from a central axis.

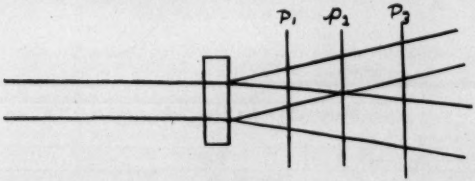


FIG. 5.

Now the separation of the images will entirely depend on the distance of the plane in which they are formed from the prism, whose angle of deviation (α) is fixed. At P_1 P_2 and P_3 we will have varying interspaces. Next, let us take two luminous points. This offers several variants. We can—

(1) Move the plane backwards and forwards until we obtain contact as at P_2.

(2) Which is much the same, we can fix the image plane and move the prism backwards and forwards.

(3) We may fix the distances of plane and prism, and modify the angle of deviation (Helmholtz).

(4) We may fix the plane of the image, the plane of the prism, and the deviation of the prism, in which case the only way to alter the separation of the images is by altering the distance of the luminous points.

Thus, if we have two points, O and X, imaged by such a system.

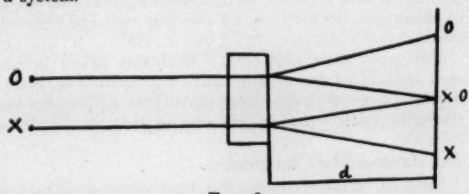


FIG. 6.

Here at the moment of contact the angular separation is exactly equal to the space between O and X.

This is the principle utilised by Javal and Schïotz. They used a doubly refracting prism (Wollaston's). It would be out of place to go into the various methods of producing doubled images; they are very numerous, consisting of bi-plates, bi-prisms, split lenses, and doubly refracting substances such as calcite.

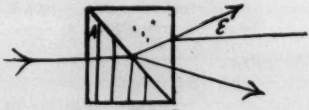


FIG. 7.

The Wollaston Prism.

The Wollaston prism is a compound prism made up of two rectangular prisms of quartz so cemented by their hypotenuses as to form a parallelopiped. The component prisms are so arranged that the optic axis of the first prism is parallel to the first face, while the section of the other is perpendicular to the axis.

A ray incident to the first face normally travels along

the normal, but is really two rays, an extraordinary and an ordinary, the ordinary with a velocity v_o, and the extraordinary with a velocity v^e. On reaching the interface the ordinary ray is refracted extraordinarily, since the principal planes are at right angles, and the angle of emergence (ϵ) is given by the equation—

$$\text{Sin } \gamma = (\mu_o - \mu^e) \tan A.$$

The extraordinary ray traverses the normal of the first prism with a velocity v^e, but becomes an ordinary ray in the second prism with velocity v_o, hence its angle of emergence is exactly as for the first ray, and they are symmetrically deviated from the normal.

The great advantage of the Wollaston prism is that each individual ray of the luminous object is split into two, and symmetrically deviated, so that we produce two emergent cones equal in size and of half the luminous intensity.

The Javal-Schiotz Instrument.

Now, in this instrument, the idea aimed at is to suppress all calculation and to make the arc read directly the power of the cornea in diopters and its radius in millimeters.

$$\text{Since } D = \frac{1}{F_1} \text{ and } \mu = 1.3375$$

$$D = \frac{\mu - 1}{R} \text{ and } R = \frac{\mu - 1}{D}$$

$$i.e., \; D = \frac{337.5}{R} \text{ and } R = \frac{337.5}{D.}$$

In our formula—

$$\frac{0}{1} = \frac{2d}{R} \text{ we can substitute for R}$$

$$\frac{0}{1} = \frac{2d}{\frac{337.5}{D}} = \frac{2dD}{337.5}$$

$$0 = \frac{2dD1}{337.5}$$

Graduating the Arc.

And here I = the image, which, when contact is obtained by the fixed prism in the fixed plane, is equal to the doubling. Now, in graduating the arc, we are going to make each degree of angular movement equal to 1 D., therefore, the object corresponding to I must have a size of x diopters.

$$\therefore x\mathrm{D} = \frac{2d\mathrm{DI}}{337.5}$$

$$\text{And } x = \frac{2d\mathrm{I}}{337.5}$$

And as x is to be 1° long we have—

$$\frac{1^\circ}{360^\circ} = \frac{x}{2\pi d}$$

$$\therefore \quad x = \frac{2\pi d}{360} = \frac{2d\mathrm{I}}{337.5}$$

And the doubling will be—

$$\mathrm{I} = \pi \frac{337.5}{360} = 2{\cdot}94 \text{ mm.}$$

So that to make 1° of arc = 1 D. the doubling of the prism at the distance of the ocular plane must be 2.94 mm. To get the radius of the arc we have to select the linear value of a degree, say we wish to have it 5 mm. then we have—

$$\frac{2\pi\mathrm{R}}{360} = 5 \text{ mm.}$$

$$\text{and R} = 30 \text{ c.m.}$$

The telescope simply picks up, as it were, the corneal images and transfers them (inverted) to the plane of the eye-piece, so that it is the same as if we took a low power microscope and examined the corneal images in the same way as one examines an opacity by means of a lens. To effect this, when the instrument is in focus, the corneal image lies at the anterior focus of the front objective, and, consequently, the emergent rays are parallel. The second objective re-unites them into an image in its

principal focal plane. In the parallel interspace we place the prism. When we obtain contact by moving the mires along the arc we know that the image measures exactly 2.94 mm., and that the reciprocal object measured on the arc expresses the dioptric power of the cornea in terms of the angular separation of the mires.

If the cornea is astigmatic, with its meridians at right angles to each other, we will find that contact taking place in the one meridian is "out" for the other. If the second meridian is of greater curvature, the images will be smaller and crossed, while if its curvature is less the images will be larger and apart. Hence, we can measure the power in one meridian and compare it directly with the power in the second.

To obviate the necessity of readjustment of the mires in the second meridian, the mires are constructed in steps, at least, one of them is. Since 1 D. = 5 mm., each step is 5 mm., and the overlapping is exactly equal to the increased breadth necessary to give the object to restore contact.

Axis.

When the arc is situated in one of the principal meridians, the images are on the same level and the black lines in the mires are coincident. But if the meridian is a secondary one we find that the axes are separated, they no longer lie in the same plane.

A simple experiment will illustrate the point. Take a white card, and on it make a bold, black cross. Then hold the card in front of the back of a teaspoon so that you can view the reflected image. When in the longest and shortest axes of the spoon, the lines of the image coincide with those of the card. Next, hold the card obliquely. It will then be found that the axes of the image are at a different angle. They are twisted towards one or another of the principal meridians.

The cornea is such an ellipsoidal mirror. In a secondary meridian, the corneal images are twisted away from the meridian of the arc, but as their doubling by the prism is always in the plane of the arc it is clear that the black lines must be separated tangentially. One turns the arc then, until the black lines of the mires pass through the same plane when we know that we are on one of the axes of the astigmatic surface.

Relationship Between Ophthalmometric and Optometric Astigmatism.

As a general rule it will be found that the emmetropic eye shows with the ophthalmometer a slightly increased curvature in the vertical meridian. There is no subjective astigmatism, and this amount observed (0.75 D.) is termed the physiological degree.

It is equally common to find subjectively astigmatic eyes more curved in the vertical, and this deviation is, therefore, known as "astigmatism with the rule." Much more rarely the corneal ellipse is prolate, that is to say, the meridian of greatest refraction is horizontal, = "astigmatism against the rule."

The following are the practical issues of a large number of observations:—

a. When the ophthalmometer shows no astigmatism at all, there is subjective astigmatism "against the rule" of 0.5 D.

b. When the ophthalmometer shows astigmatism "against the rule" the subjective amount is greater.

c. When the ophthalmometer shows 0.75 D. with the rule, there is no subjective astigmatism.

d. When the ophthalmometer shows astigmatism lower than 3 D. "with the rule," the subjective amount is the same.

e. When the ophthalmometer shows astigmatism over 3 D. "with the rule," the total amount is rather more.

Explanation of Differences.

1. The main factor appears to be the posterior surface of the cornea. The difference of radii will be as marked for the posterior as for the anterior surface. But since in its relation to the aqueous the cornea acts as a negative toric lens, the astigmatism, which in the front face was with the rule, will, for the back face, be against the rule, and so tend to neutralise the error. This would explain the physiological astigmatism.

2. Obliquity of the lens. A convex lens, when tilted so that rays are incident on a secondary axis, becomes astigmatic, and the focal plane nearest the lens is that parallel to the axis of rotation. Any obliquity of the crystalline lens would, therefore, account for some of the difference.

3. The distance of the correcting lens from the eye. The ophthalmometer measures the lens correcting the astigmatism, which would have to be worn in contact with the cornea. But folks could hardly wear a toric balsamed on their corneas.

The lens has to be brought to the anterior focal plane, 13 mm. in front.

Thus, a cyl. of + 6.0 D. on the cornea would be + 5.5 in the trial frame.

$$\text{for } f = \frac{1000}{6} = 166.6 \text{ mm.}$$

$$\text{and } \frac{1000}{166.6 + 13.0} = 5.5 \text{ D.}$$

Likewise, a cylinder of − 6 on the cornea becomes − 6.46 D. in the trial frame.

$$f = \frac{1000}{6} = 166.6 \text{ and } \frac{1000}{166.6 - 15} = -6.46 \text{ D.}$$

4. Thomas Young first showed that the refraction throughout the pupillary area is not identical, so that it is open to question whether there can be such a condition as a *perfect* optical correction. Any correction is more or less of the character as the best approximation to the mean.

5. Astigmatic accommodation might also account for the difference. So far no clear case has been demonstrated, but there is nothing impossible in the assumption. Hensen and Voelckers found that by stimulating with a galvanic current an isolated twig of the ciliary nerve, they could produce a localised contraction of the ciliary muscle, and, therefore, an astigmatism of the lens face.

We note also that the ciliary muscle has its own little nervous ganglion from which a network of fibres passes to the various points in the muscular ring. Such a mechanism would make selective action possible.

This brings me to a conclusion of this paper, and it only remains for me to thank you very warmly for the kind attention with which you have listened to me.

Mr. Curties said he was sure they were all very

thankful to Dr. Ettles for the lucid manner in which he had tackled rather a difficult subject. Being somewhat connected with the makers of ophthalmoscopes, he could tell them that, thanks to Dr. Ettles' skill, the instrument they saw was a perfect one, and rightly enough Dr. Ettles was very proud of it. He would like to congratulate him upon the model, which was a splendid one for showing the principle of the ophthalmoscope to those who did not understand it.

MR. ROSENHAIN had listened with great pleasure to Dr. Ettles' account of the ophthalmometer, the more so because he knew next to nothing of these instruments when he came there that evening, while now he could flatter himself that he did know a little about them—and that was always a pleasant feeling. There was just one point that had occurred to him, namely, the figures given for the refractive indices of the various media of the eye. Two of these, at least, were liquids, one pasty, or semi-liquid, and all of them composed of complicated organic materials. Dr. Ettles gave the refractive indices to four places of decimals. He (Mr. Rosenhain) had had some occasion to deal with measurements of refractive indices of liquids, and he had found that it was exceedingly difficult to get anything like the degree of accuracy indicated by those figures. The temperature co-efficient of liquids in this respect was very considerable, making a difference that ran (he was speaking from memory) into the third place of decimals, he therefore thought it would be wiser to state the values for the media of the eye to three places only. He should imagine that these figures would differ quite perceptibly from one individual to another, while the actual refractive index at the temperature of the human body was quite decidedly different from that determined when the liquid was extracted and placed upon a glass prism—that involved a difference of some 30 degrees F. in temperature. He would be glad to know whether these things had been considered, and if not he thought they would be well worth considering.

MR. CHALMERS was very much interested in Dr. Ettles' lecture and description of the Ettles-Curties form of ophthalmometer. He wished to ask what accuracy Dr. Ettles thought was possible using every precaution. His (Mr. Chalmers') experience was that it was very

difficult to measure curvatures even of lenses with as great accuracy as was generally claimed, and these difficulties seemed greater in the measurement of the cornea.

He thought that the description of ophthalmometers would be simplified if some appropriate word could be found for the microscope or telescope, which would denote that it was really a microscope of very low power.

Dr. Ettles said he was exceedingly obliged to them for the kind way in which they had listened to him. Mr. Chalmers had raised a point of great interest, namely, in regard to the question as to whether the ophthalmometer was a microscope or a telescope. He had used the word telescope, and he had hoped that they would let it go at that. What happened was this. The image was transferred to the focal plane of the eyepiece, and here it had exactly the same dimensions as the corneal image, but was inverted. So far there was no magnification, and the Ramsden eyepiece acted as a microscope.

In regard to the matter of accuracy, that was a very important point. One could be sure of the first decimal point in the radius, but the second decimal point was not a matter of positive certainty. Mathiessen, using the Javal-Schiotz ophthalmometer, and comparing the readings with the Helmholtz ophthalmometer, found that contrary to the general opinion he was able to get more accurate results with the Javal than with the Helmholtz, but with neither could one be quite certain of the second decimal figure. Mr. Chalmers asked what sort of variations would be obtained. None that could be appreciated, except one instrument was checked against another in the same eyes. The accuracy of the contact of the doubled images in the plane of the ocular was a point of considerable difficulty, partly because one got a dispersion with any prism, and with the Javal, amongst others, borders of the images *irisés*, as the French called them, were observable. That error had been eliminated to some extent by using light more or less homogeneous —say, red light and green light, because the dispersion with that sort of illumination was less than with other forms. The error was not a very significant one. The deviation for red light was slightly different from the deviation for green light. Then, again, there was the chromatic aberration in our eyes.

Mr. Chalmers suggested that two shades of the same colour might be used.

Dr. Ettles said he had been using orange glass to get as much luminosity as he could, but it was too impure.

Mr. Rosenhain suggested the use of a Cooper-Hewitt lamp.

Dr. Ettles said that might be done, but as it was a clinical instrument it would add to the expense. Proceeding, he said he was indebted to Mr. Rosenhain for two very useful suggestions, especially the one about the temperature of the refractive media. The figures he had used were principally those of Mathiessen, who investigated the indices of the refractive media of the eye, and published them in the 'Festschrift' in honour of Helmholtz's 70th birthday. The later ones were those of Stadfeldt, who had published a paper on the index of the crystalline lens and media. Dr. Ettles considered that the refractive indices should be re-determined, and he would bear in mind Mr. Rosenhain's suggestion.

ONE-POSITION OPHTHALMOMETRY.*

By John H. Sutcliffe, F.R.S.L.

Read before the Optical Society on March 21st, 1907.

The Earliest Instrument.—The science of ophthalmometry is by no means new, for we find that even in 1664 Pourfour du Petit, a French physician, invented an in-

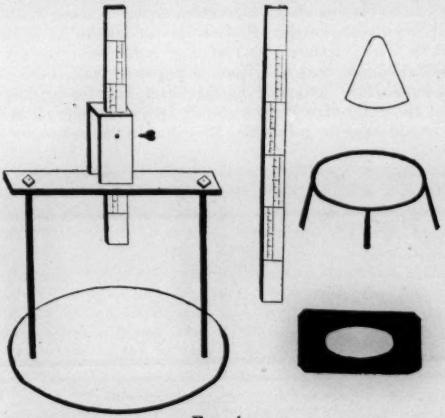


Fig. 1.

Pourfour du Petit's Ophthalmometer. This Sketch is taken from an Old Print now in the Bibliotheque Nationale at Paris.

strument in appearance rather like an ordinary spherometer. (Fig. 1.) I think that here is the first time this sketch has been shown in this country. This could only be used on the dead eye. It is a long stretch from 1664

* Lecture given with a practical demonstration upon a new form of ophthalmometer.

to 1907; naturally during that time wonderful improvements have been made. Kohlrausch and Helmholtz developed the instrument into real utility in the laboratory. Coccius, Javal, Schiötz, and a little band of workers at the Sorbonne made it of practical use in the clinic, while Inskeep introduced many excellent mechanical improvements. I have now to show an instrument which has the honour of seeing light for the first time to-night.

The Principles of Ophthalmometry.—I will content myself with the briefest possible "lay" description of ophthalmometric principles. Let us treat the human

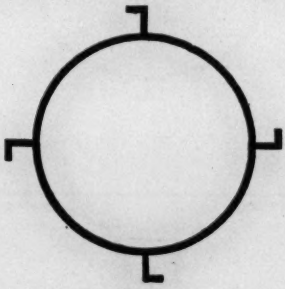


FIG. 2.

IMAGE OF THE "SUTCLIFFE" MIRE AS SEEN ON A NON-ASTIGMATIC CORNEA.

eye as a convex mirror. Any object reflected in it will appear very much smaller. If our object is circular and the mirror spherical, the image will be circular; if the mirror is of an egg-shape, *i.e.*, longer in one diameter than another, the image will be oval instead of being round. The object of the ophthalmometer is to measure the difference between the length and breadth of this oval as seen in an astigmatic patient. If the eye is truly spherical, the image is not oval; in such a case the ophthalmometer serves no useful purpose, beyond proving the absence of astigmatism. It is a well-known optical fact that if we know the size of an object, its distance from the convex eye or mirror, and

also the size of the image, then we know the curvature of the eye. Our aim is to ascertain the length and breadth of the image.

Why Doubling is used in Ophthalmometry.—All scientists are agreed that the best way to measure the size of this image is to employ the principle of doubling, the principle of which I will explain. Suppose fig. 2 to be

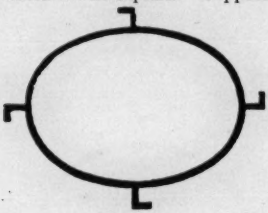


FIG. 3.

IMAGE SEEN IN A PATIENT WITH HIGH ASTIGMATISM "WITH THE RULE."

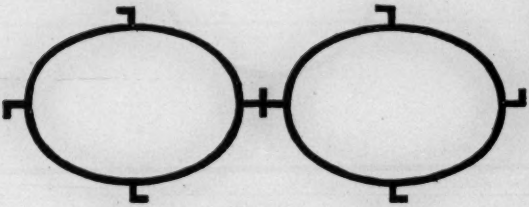


FIG. 4.

IMAGE OF FIG. 3, BUT DOUBLED HORIZONTALLY AND IN CONTACT KNOWN AS FIRST POSITION.

the reduced circular image of a large circular object, as reflected in the eyes of a patient. This is known as the "mire." Fig. 2 shows the pattern of the mire of my instrument. If the eye of the patient is egg-shaped or astigmatic, say with the length of the egg in a horizontal position, the image will appear as in fig. 3. Let us double it in a horizontal direction, as in fig. 4, just so that the long ends are in contact. Now swing round

the two ovals, still keeping the length of the oval at a horizontal axis, and we have fig. 5, the ovals being separated. Imagine that by some optical method we can make the broad sides of the two ovals touch, then we can estimate the mechanical work required to do that as being the difference between the length and breadth of the oval. It might happen that the egg or oval might not be lying in a position horizontal, but in a diagonal or oblique one, in which case the images would appear

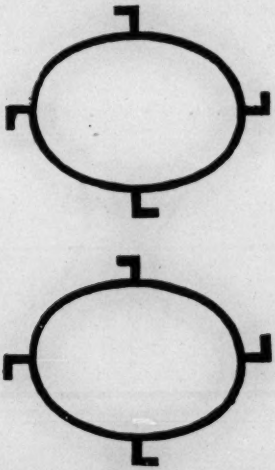


FIG. 5.

IMAGES IN FIG. 4, SWUNG ROUND TO OPPOSITE AXIS, OR SECONDARY POSITION.

as in fig. 6, out of alignment. If we again swing them round, we shall have them as in fig. 7, not in contact, but in alignment. We can make them approach each other as in fig. 4. By swinging round to the opposite axis we prepare for the secondary position, fig. 8, which is completed in fig. 9. The axis is thus found by alignment, the astigmatism by the mechanical amount

of work performed by the instrument in approximation or separation. This gives a very rough idea of how the axis and the amount of astigmatism are found.

The Utility of Ophthalmometry.—I do not propose to enter into discussion of the advantages or disadvantages of ophthalmometry. My object in demonstrating this instrument is solely to show that ophthalmometry, as it at present exists, is capable of speedier and more accurate manipulation. It has been said that astigmatism is almost entirely corneal, and that the ophthalmometric axis is almost invariably the subjective one. My own opinion is that it can be proved almost invariably that any differences between the subjective and ophthalmometric findings can be traced to either a faulty instrument or to a careless observer, and rough subjective

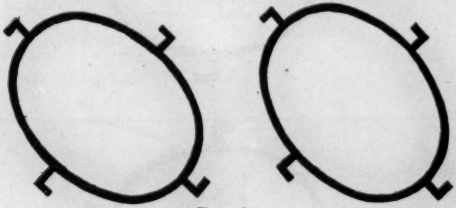


FIG. 6.

DOUBLED IMAGES IN ASTIGMATISM WITH A DIAGONAL AXIS, BEFORE FINDING FIRST POSITION.

testing. It is one thing to work in a laboratory and quite another in a consulting room. We have said that the following three points in the ophthalmometric formula *must* be known :—

1. The size of the object.
2. The size of the image.
3. The distance of object from image.

A well-constructed instrument and a careful observer will give us the first two essentials. The last requirement is rarely fulfilled, and is, therefore, the cause of the trouble.

Why Ophthalmometry is generally Unsatisfactory.—This absolutely necessary optical requirement of the

formula of the ophthalmometer may be summarised, viz :—

(*a*) That the observer shall be able to make the instrumental focal distance an absolutely exact one.

(*b*) That the distance of the eye of the patient shall not alter in the slightest degree when taking the two measurements.

These two points have been bugbears of successful work. I hope to show you that I have for all practical purposes overcome them both. I propose to discuss each part of the new instrument separately, giving particulars of

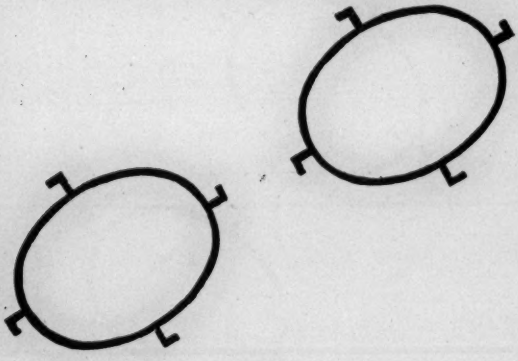


FIG. 7.

IMAGES IN FIG. 6, NOW IN ALIGNMENT, BUT NOT IN CONTACT.

other models when I have deemed it necessary to depart from standard patterns.

The Construction of the usual Ophthalmometer.—The general construction of an ophthalmometer requires :—

1. A support of some kind to steady the head of the patient.
2. An object, generally illuminated, termed a mire.
3. A telescope and eyepiece through which the observer may enlarge the image seen in the patient's eye.

4. The mechanical means for adjusting the telescope.
5. Some optical system of doubling and approximation, generally placed between the two objectives of the telescope.

My own instrument naturally follows the above lines.

The Mire.—My mire (I prefer the English pronunciation with the long *i*) is a circle, at four points of which are L-shaped hooks as in fig. 2. It is situated at about

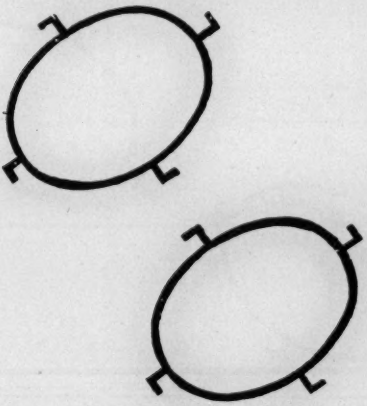


FIG. 8.

IMAGES IN FIG. 7 READY FOR APPROXIMATION TO SECONDARY POSITION.

125 mm. from the eye of the patient, is about 100 mm. in diameter, and forms an image slightly less than 3 mm. on the cornea. I am indebted for the form of this mire to Mr. Rudyard Kipling, who has unconsciously furnished me with the design. Fig. 10 shows a copy of his monogram. You will notice that I have reversed the order and put the hooks outside the circle. The diameter is much smaller than the usual type. There is an

increased sensitiveness of focus owing to the proximity of the mire, which is more than compensated for by compactness and the increased illumination. The pattern is cut out of metal and behind the opaque glass are fixed the usual electric lamps.

The distance between the ends of the hooks is the diameter of the mire. The horizontal parts of the hooks, fig. 27, are used for securing alignment (axis directions), the vertical ends of the hooks are for the purpose of obtaining "contact," fig. 28. The inner circle does not

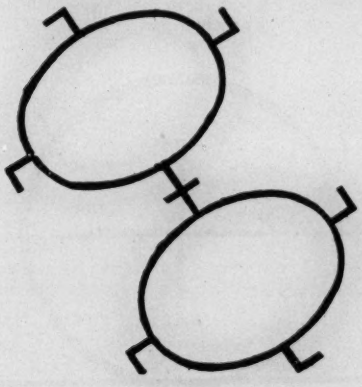


FIG. 9.

IMAGES IN FIG. 8 IN ALIGNMENT AND CONTACT, SECONDARY POSITION.

serve any purpose in ordinary cases of astigmatism beyond giving a definite form and illumination to the image in order that it may be easily found by the observer. It is, however, of use in high degrees of curvature, and also in keratoconus. It is important in all ophthalmometers that there should be no easily disorganised mechanism in connection with the mire, and for that reason there should be an advantage with this pattern in that there is absolutely nothing to disturb or

get out of order. The lamps are "commercial," and can be used of low voltage for a battery, or with higher voltages for a city current. The hooks show "touch" when doubled; they are not on the customary "contact" principle, but on that of "opposite pointers."

My reasons for adopting this form of mire are deduced from the two methods employed in ophthalmometry for finding the exact place at which the double images appear to approximate, viz.:

(*a*) The "contact" on which the "step" mires are based. The drawbacks to this system are very great. If a square object is reflected into a convex mirror the sides of the image will be seen to

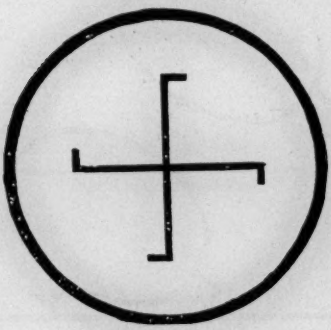


FIG. 10.

MR. KIPLING'S MONOGRAM.

be distorted, the corners of the square being drawn towards the centre (fig. 11). I show you here a diagram distorted owing to reflection, and also one where the usual parallelogram (fig. 12) or "step," is similarly distorted (fig. 13). Thus we get two curves, the ends of which cannot form contact at the same time as the centre. The question as to which of these two points must be considered as contact is difficult to decide; moreover, these particular distortions alter with each vary-

ing curve of the eye. Another drawback is that it is an extremely difficult matter for any but a careful observer to decide even with straight-sided "steps" as to when "contact" takes place owing to the "nimbus" of light radiating from the illuminated mires. There is present a kind of halo that obscures the actual sharp outline of the mire. Add to this the distortion I have spoken of above, and you will see that there are objections to any mire where the images depend upon "contact" for approximation. In the old days before translucent mires came into use, this "nimbus" was not noticeable.

FIG. 11.

DIAGRAM ILLUSTRATING THE DISTORTION OF A SQUARE WHEN REFLECTED IN A CONVEX MIRROR. THE SQUARE HERE SHOWN SHOULD BE OUTSIDE THE DISTORTED FIGURE INSTEAD OF INSIDE.

(*b*) The "point to point" system as is found in the Inskeep type of instrument. This is unmeasurably superior to the first method. To illustrate this, I show you two straight electric candle lamps, placed end-to-end, or tip-to-tip, above one another. It is quite an easy matter to decide when these points are opposite to each other. It is due to this fact that the Inskeep instrument undoubtedly ranks as being one of the best of double position ophthalmometers. Compare this with the two curved electric lamps I place side by side with the convex surface either in or out. You will notice that it is difficult to tell when they nearly touch, actually touch, or are slightly overlapped, all due to the "halo" or "nimbus." How much more difficult, then, when one is in doubt as to whether the ends contact.

You will notice that in fig. 14 I show a similar curve or distortion present in my own model, but as I use only the parts on the horizontal lines this distortion does not make the slightest difference. There is no distortion with the alignment lines.

The Doubling and the Approximation.—By doubling is meant the method by which the original image is doubled for the purposes of measurement. By "approximation" the method by which the doubled mire images are

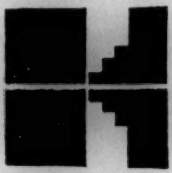


FIG. 12.

THE "STEP" OR PARALLELOGRAM MIRE UNDISTORTED, BEFORE REFLECTION.

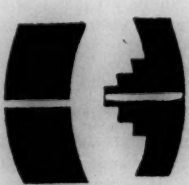


FIG. 13.

THE "STEP" MIRE DISTORTED, AFTER REFLECTION.

made to separate from or to approach each other. This doubling, and approximation, is of three kinds:

(*a*) Where the doubling is a fixture and the approximation is secured by altering the size of the object. This alteration in the size of the object is usually secured by moving one or both of the mires laterally (the Javal model) or else by decreasing the

size of the object by a diaphragm as in the Reid model.

(*b*) Those where the doubling alters and so secures the approximation, and the mire as an object remains unaltered.

(*c*) Where the doubling is stationary and the mire is fixed, but where the approximation is effected by moving the mire to and fro. This is an old model of my own which I have since discarded.

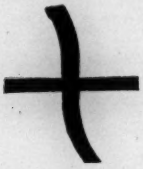


FIG. 14.

FIGURE SHOWING DISTORTION OF CONTACT LINES, BUT NOT OF ALIGNMENT LINES.

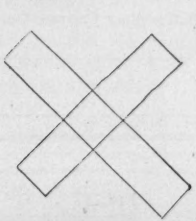


FIG. 15.

THE HELMHOLTZ INCLINED THICK PLATES OF GLASS.

My own objections to the first and the third kinds are that the size of the image remains the same whatever may be the curve of the cornea; thus a different part of the pupil, a comparatively wider one than the ideal diameter will be measured with a small emmetropic cornea; while a narrower image than 3 mm. will be made with a large emmetropic cornea. There is an objec-

tion to instruments where only one of the mires moves in that thus a decentered position of the cornea is measured, while an objection to instruments where both mires move, is that the very slightest movement of the mires is sufficient to account for a large degree of astigmatism. Another objection to movable mires is that the attention of the patient is distracted not only between the measurement of the two positions, but also in the measurement of *each* position.

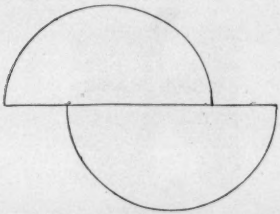


FIG. 16.

SPLIT DECENTERED CONVEX OBJECTIVE.

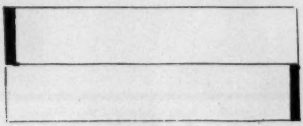


FIG. 17.

PRISMS BASES OPPOSITE.

The second kind, having stationary mires, does not possess these drawbacks. To this class belong those which effect the approximation by means of moving prisms. My model has a fixed mire, the size of the image being different with every varying curvature. The size of this image is proportionate to the curve of the cornea.

The doubling arrangements in ophthalmometry invariably consist of an objective across the face of which

is placed some combination of prisms whereby the original image is split into other images of less intensity. The methods of doubling may be classified as under. (It is understood that in these examples we speak of a horizontal doubling).

1st. The single horizontal split.

(*a*) Two plates of thick glass set at opposite angles. Fig. 15 (Helmholtz).

(*b*) A lens with split halves, one decentered to the right, the lower half to the left. Fig. 16.

(*c*) Two prisms, one base to the right, the other to the left. Fig. 17.

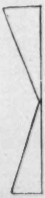


FIG. 18.

PRISMS SET WITH VERTICAL LINE OF SEPARATION. THESE ARE SOMETIMES INCLINED.

Although there is good illumination with this pattern the readings of the instrument will be hopelessly wrong if the focusing is not exact. The alignment for securing the axis is very uncertain.

2nd. The single vertical split.

(*a*) A pair of prisms, bases, or apices together, the dividing line being vertical. Fig. 18.

(*b*) A lens out of which a centre slab has been taken, the two outer halves joined, the slit being vertical. Fig. 19.

(*c*) Two plates set at an angle with the dividing line vertical. Fig. 20. (Hardy model.)

(*d*) Two lenses from which the outer edge has been cut, leaving two decentered lenses, junction vertical. Fig. 21.

The disadvantages are that, as with the first kind, the slightest inaccuracy in focus gives an error easily amounting to over one diopter. The alignment, however, is always correct.

All patterns where the original objective is split call for an almost impossible nicety of adjustment.

Pattern (*c*) is undoubtedly the best of these; patterns

FIG. 19.

TWO CONVEX LENSES DECENTERED AND JOINED TOGETHER.

(*b* and *d*) are almost impossible, requiring nicety of manufacture; pattern (*a*) is used in the Kagenaar model.

3rd. The equal-coned split.

Under this heading come the Coccius prism and the Wollaston prism. Fig. 22.

FIG. 20.

INCLINED PLATES (HARDY).

This system has a correct alignment, and has not the decided and palpable errors of inaccuracy if the instrument is only a trifle out of focus. Anyone, however, but an extremely careful observer is likely to make miscalculations owing to the varying separation and approximation due to inaccuracy of focusing. Personally, I do not like the Wollaston prisms owing to the comparative losses of light and brilliancy of image. It is incident-

ally difficult to secure good prisms unless of a rather costly nature. The ordinary commercial Wollaston prism as found in some of the cheaper instruments is full of chromatism and absorbs an undue quantity of light.

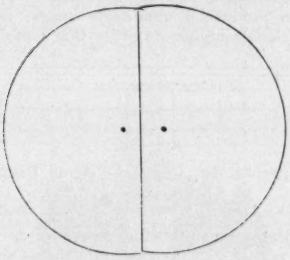


FIG. 21.

TWO LENSES. THE DOTTED LINE SHOWS THE CENTRES.

3rd. The Rotating Prism.

This is practically a Herschel, Cretes, or Risley prism; the two prisms revolve over each other's surface in opposite directions, causing the image to move in a straight line. If in the centre of these two prisms is bored a hole, we shall have two images, one fixed and the

FIG. 22.

THE WOLLASTON PRISM.

other movable. I went to the expense of a model some time ago, fondly imagined myself its inventor, until I found that Landolt had been good enough to design the same model about twenty years previously. It was discarded by me on account of requiring delicacy in manu-

facture, and also because I could obtain only the ordinary double-position ophthalmometer.

4th. The double-split with oblique decentration.

I have constructed quite a new arrangement of prisms to effect the doubling and the approximation. My first idea was to produce a quadruple image, or rather a duplicated double image, in order that I could measure

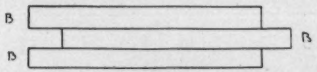


FIG. 23.

THREE CYLINDERS, THE CENTRE PORTION DECENTERED.—
—COMBINATION A.

both meridians without being compelled to swing the arc round and so distract the attention of the patient. To effect this I slit in half a weak spherical lens, displacing both halves, thus securing a horizontal decen-

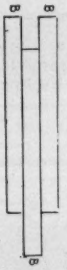


FIG. 24.

COMBINATION B.

tration and movable doubled images. At right angles to these I placed a similar combination; this worked well but for the drawbacks previously mentioned of faulty alignment and adjustment, unless the instrument was in exact focus. It was again almost impossible to split

and edge a spherical lens horizontally without also introducing a vertical decentration. I next tried a split combination where the upper half was + 0.25 and the lower half − 0.25; by bodily sliding this transversely to the plane of the objectives, two images were produced moving in opposite directions, the combination, of course, being neutralised by other half lenses. The same objections as above, however, applied. In fact, I have on the

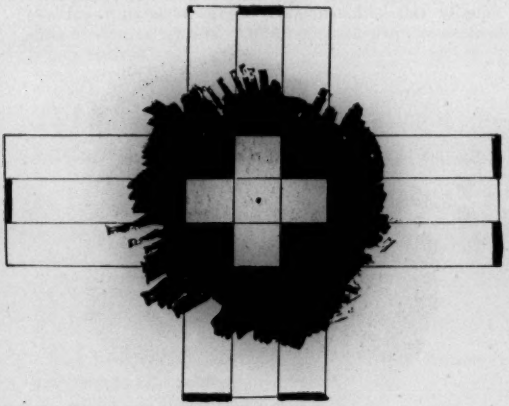


FIG. 25.

SIX CYLINDERS. FIGS 23 AND 24 SUPERIMPOSED AT RIGHT ANGLES. THE CENTRE CYLINDRICAL PRISM IN EACH CASE SET OPPOSITE TO THE TWO OUTSIDE ONES.

shelves of my laboratory considerably over fifty models of methods by which doubling might be effected, and yet none of them have ever been used in an ophthalmometer. And they never will be! All of them have faults resulting in faulty alignment and contact.

It has been said that "The Better is the enemy of the Good." In my case I had almost begun to despair; so many "better" ideas occurred, resulting in the destruction of the old "good" ones, that there seemed a possi-

bility that the "one-position" ophthalmometer would never materialise. Finally, after long experiments, with which I will not worry you to-night, but hope to lay before you at some other time or place, as a matter of historical experiment, I decided upon the present system I have to show you.

This, I claim, gives an accurate position of focus, an easily adjusted method of doubling, and a "coarse" system of approximation, together with the power to read equally well both meridians at the same time. As my

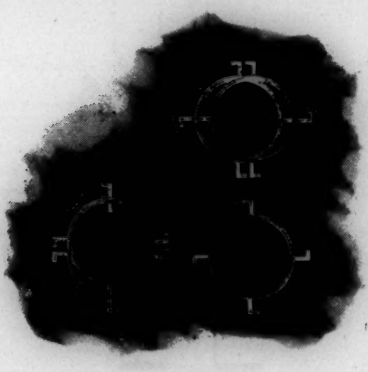


FIG. 26.

TO SHOW THE QUINTUPLE IMAGE PRODUCED BY THE SLIGHTEST INACCURACY OF FOCUS.

doubling system I have used a very weak element + 0.06 D., and split it up, decentering and moving various portions. I take a piece of + 0.06 cylinder (A) about 20 cm. long and 35 mm. wide. The axis of the cylinder is across the narrow portion and at right angles to its length. If we take another piece (B), similar in every way, but place it over the other and at right angles to it, we have where they cover each other a + 0.06 spherical lens. Holding this in the form of a cross, and looking through the spherical part, if we move one cylinder ver-

tically, the image moves vertically; if we move the horizontal cylinder, the image moves horizontally. Now let us split longitudinally the cylinder A into three equal parts, and move the centre one out or in. We shall have two horizontal images, one of which is fixed, the other movable. Fig. 23.

Let us also split in similar fashion the other cylinder (B), the three cylinders, this being at right angles to the other three, fig. 24. The two combinations, A and B, are now superimposed at right angles to each other with an effect similar to fig. 25. All except the five centre squares is now blocked out.

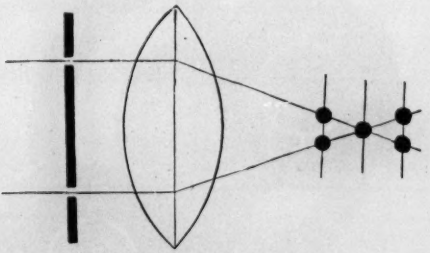


FIG. 27.

THE SCHEINER EXPERIMENT, SHOWING THE PRINCIPLE UPON WHICH THE FOCUSING SYSTEM IS BASED.

THE TRIPLE IMAGE.—If we place this six-cylinder combination in front of an objective, we shall have three images when the combination is in focus; the original image being displaced, 1st, down and to the left; 2nd, up and to the right; 3rd, down and to the right.

WITH ABSOLUTE FOCUS.—If we move the horizontal cylinder the top and corner images will move horizontally together; if we move the vertical cylinder the two bottom images will move together in a vertical direction. Not only can one thus secure touch or contact in both meridians simultaneously, without any rotation of the mires, thus abolishing the second position, but a new and important element comes into force; if we are not at an absolute focus the two outside images become doubled,

thus making five instead of three. Fig. 26. This is due to an old and well-known principle seen in what is known as the Scheiner experiment. Fig. 27. If we stop up the whole of an objective except two holes at some distance from the centre, we shall have one image when the lens is in absolute focus and two when otherwise. This principle is used in the Thomas Young optometer. The slit taken out of the middle of my combination is equivalent to a slab of opaque material, and thus the two

Fig. 28.

Fig. 29.

outside lenses give a double image when not in absolute focus.

I attach considerable importance to this focusing arrangement, because, as I have before stated, not only is it necessary to have a clear focus, but the position must be exact, otherwise the formula of ophthalmometry is disturbed. It is not enough that the observer sees the image clearly, he should see it at its correct position.

The Telescope.—The telescope and eye-piece do not present much that is new: the front lens is about 10

inches focus, the back one about 15 inches. The convex surfaces of the two achromats are together. The eye-piece is an ordinary one. The cylinders are placed between the two achromats. The front lens is placed in its focal distance from the patient, the rays then proceed parallel through the cylinders, and are collected by the back lens and transmitted to the eye-piece.

The Differential Gear and Mechanism. — The mechanism for moving the centre cylinder of each triple

Fig. 30.

combination consists of two slides on opposite sides of a brass plate. Each slide carries two of the centre cylinders, the inside cylinders remaining stationary. The slides are drawn by cog-wheels working over a common yet independent centre. One slide is connected with a dial on which the differential figures are inscribed, the other is attached to a pointer. By this means, not only are the separate curvatures of each meridian recorded, but in addition the dial records the actual difference between the two curves, *e.g.*, curves of 47.25 and 49.25 would be registered as 2.00 diopters; 43.25 and 41.25 would also appear as 2.00 D. There is the further advantage that the instrument actually registers whether the astigmatism is "with" or "against" the rule. The

actual result is thus recorded without any calculation on the part of the operator (fig. 30).

The axis marking arrangement consists solely of a mark on the telescope tube, pointing to a degree scale engraved on the edges of a slot in the carrying body. The telescope is revolved by a lever or handle. In no case is it rotated above 45 degrees to either side.

THE STAND AND GENERAL DESIGN.—The design of the instrument in no way resembles the usual ophthalmo-

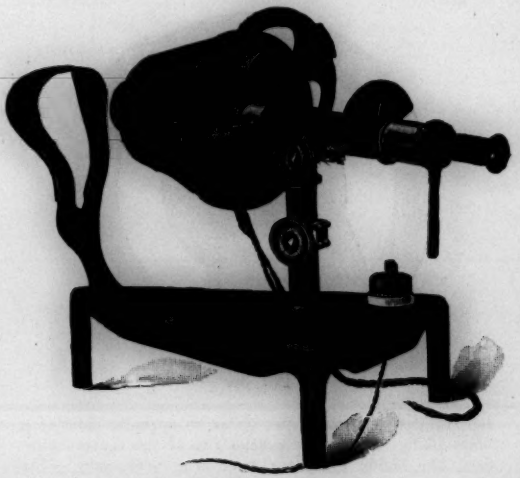


FIG. 31.

meter. I have discarded all existing models as being in any way a pattern, for the matter of that, have not followed any other kind of instrument. I was much impressed at the recent Optical Convention by a paper read there on the tendency to a slavish following of traditional patterns in instrumental construction.

The instrument before you is unconventional and might, by some, be termed ugly; but as beauty lies in

the eyes of the beholder, I trust I shall be forgiven if I say that to me it does appear "a joy for ever" (fig. 31).

I have abolished the chin rest and, instead, given an eyebrow rest. If you will make a chin rest by resting

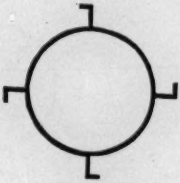


FIG. 32.
THE SINGLE MIRE BEFORE TRIPLICATION.

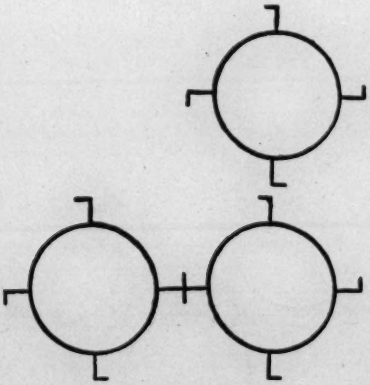


FIG. 33.
ADJUSTED FOR HORIZONTAL MERIDIAN, SEPARATED VERTICALLY.

your elbows on the table, and then resting your chin on the palms of your two hands, you will find that you have given to you a great facility for moving your eyes

about; also that the very slightest pulsation or breathing causes a movement of the upper part of the head and the eyes.

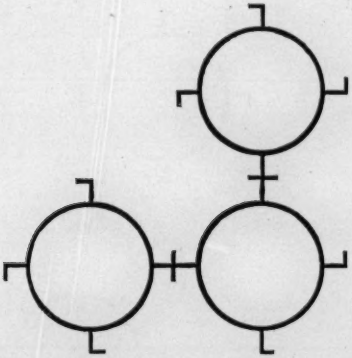


Fig. 34.

The Images Seen in Alignment, and Adjusted for Contact. Instrument at a Horizontal Axis.

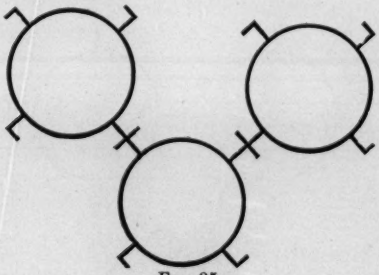


Fig. 35.

Similar to Fig. 34, but set Obliquely.

Try now instead, pressing the two palms against the forehead near the eyebrows. The movements of the chin,

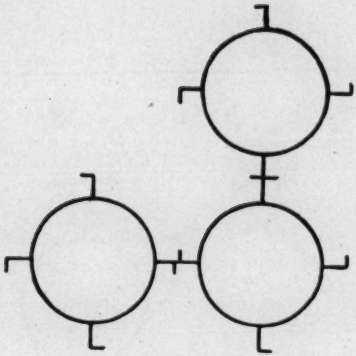


FIG. 36.
ASTIGMATISM WITH THE RULE. SHOWING AN OVERLAPPING HORIZONTALLY, BUT CONTACT VERTICALLY.

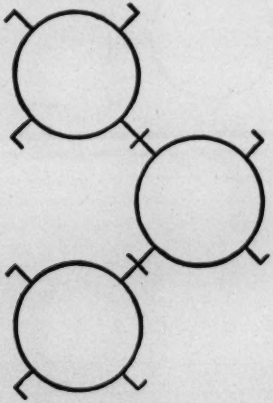


FIG. 37.
SIMILAR TO FIG. 35, BUT AT OPPOSITE AXIS.

mouth, or breathing have no effect; not only that, but the pressure of the forehead seems to restrain any desire upon part of the eyes to move. My experiments have been conducted, to some extent, upon children. I have found that the absence of the chin rest greatly increased the steadiness. The base is solid, triangular, and has three upright legs. It is intended for use on an ordinary table, and also for a special three-legged table I have designed, but this is not ready for you to-night.

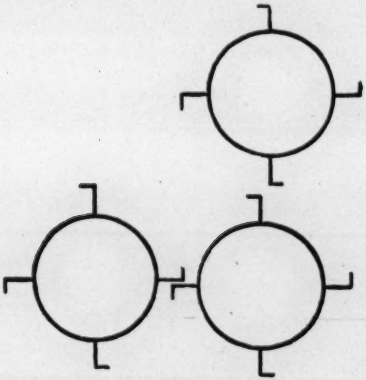


FIG. 38.

OUT OF ALIGNMENT, OVERLAPPED HORIZONTALLY, SEPARATED VERTICALLY.

The new model here is naturally subject to all the theories and laws of ophthalmometry. I do not claim that it will upset any fixed ideas as to corneal curvatures, or give us any particularly new truths. The study, I believe, will be unusually facilitated, for what was possible previously only to the experienced observer will now be within the grasp of the most casual operator. Owing to the recent construction of the instrument, I have not had time to make many lengthy experiments in further ophthalmometric research. I have found out, however, that the fact of the actual axis not being at

right angles is immediately apparent, and also that the inner ring of the mire, which is 50 mm. in diameter, and can be used, thus forms a smaller and more central image on the cornea of only 0.75 mm. from the visual line. I think there is quite sufficient material for two other separate papers upon these two points alone.

It has been my aim to construct an ophthalmometer which should abolish the secondary position, give an absolute focus, have an easily read result, and at the same time have a freedom from any mechanism that

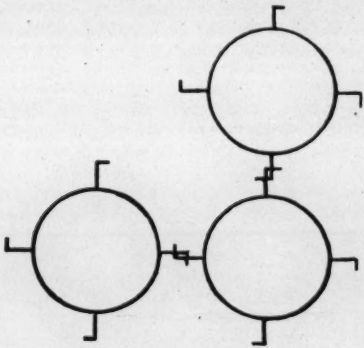


FIG. 39.

OVERLAPPED VERTICALLY AND HORIZONTALLY, OUT OF ALIGNMENT.

would distract the attention of the patient under examination. Figs. 32 to 39 are a series of triple images showing the different phases of astigmatism in and out of contact and alignment.

In conclusion I have to thank you for your attention. I trust that when you come to make a practical trial now of the instrument, that you will not have to say of me as was once said of a well-known lecturer, who announced that he had much that was "new and true" to tell them. An unkind critic said that much of the new that he had told them was not true, and *much* of the true was not new!

Discussion.

Dr. Chalmers Prentice, Mr. C. Curties, Mr. A. E. Grey and Mr. Hyatt-Woolf expressed their interest in the paper and their desire to examine the instrument exhibited.

Mr. Dixey called attention to the ingenious device to obtain coincidence, the value of the doubling when the image was out of focus, and the convenience of the one position reading.

Mr. Rosenhain congratulated the lecturer on the general design of the instrument, but called attention to the necessity of careful work in the mechanism for sliding the cylindrical lenses.

THE PRESIDENTIAL ADDRESS.

By Walter Rosenhain, B.A., B.C.E.

Delivered before the Optical Society on April 18th, 1907.

The choice of a subject for a Presidential Address can scarcely ever be an easy matter; it certainly is not so in my case. When I look back upon the list of my predecessors in this chair and at the addresses which they have contributed to the Transactions of our Society, while it makes me realise, if possible more fully than before, the value of the honour which you have bestowed upon me, it makes me realise also the difficulty of living up to the level of the standard that has been set. There is, however, one glimmer of hope in this retrospect, for I see that most of your previous Presidents have taken for the subject of their Address some branch of optics or of closely allied subjects, with which they themselves were personally best acquainted, and if I attempt to follow on somewhat similar lines I am naturally led to choose the subject of the microscope as applied to the purposes of physical science or engineering uses, as distinct from biological science and medical uses. This is a wide field, and though I may perhaps claim that my special and personal interest is co-extensive with the whole of that field, I can claim some little special knowledge only of certain portions of the subject. For the sake of completeness, however, I propose, in this address, at all events, to enumerate as many as possible of the uses of the microscope in the directions indicated, even though I cannot always speak with personal knowledge of those uses. I am further emboldened to take the course I have indicated by the consideration that in so doing I may hope in a small measure to serve that end which I take it is one of the vital functions of this Society, that is, to bring before the maker and designer of a whole class of optical instruments, the requirements of those who as men of science, or as

engineers, are the actual users of the instrument; it may even be that some who are now using less convenient methods for the attainments of their ends may be induced to try, and perhaps to adopt, the optical methods which I advocate, to the mutual advantage of their own and of the optical industry.

Microscopic Measurements.—Probably the best known and most widely used application of the microscope to physical purposes is the use of the instrument for direct

FIG. 1.

measurement; this use may be of two kinds—the microscope may be used to furnish an accurate line of sight, the magnifying power being employed more or less as an accessory for the purpose of determining the point where this line of sight crosses some fixed or movable scale. The ordinary reading microscopes attached to the graduated circles of astronomical or other instruments for the measurement of angles, and the microscopes moved by known amounts by the movement of guiding screws (*i.e.*, "micrometer microscopes"), are examples

of this type. The degree of accuracy attainable by these means, in so far as it depends upon the observing microscope, is limited rather by the coarseness or irregularity of the lines ruled upon the divided scale which is being observed than upon purely optical considerations. Figs 1 and 2 are photomicrographs showing the magnified images of lines on well-divided scales. Fig. 1 is a line on one of the standard scales of the National Physical Laboratory, magnified 800 times, while fig. 2 is a line on a good steel rule, magnified 200

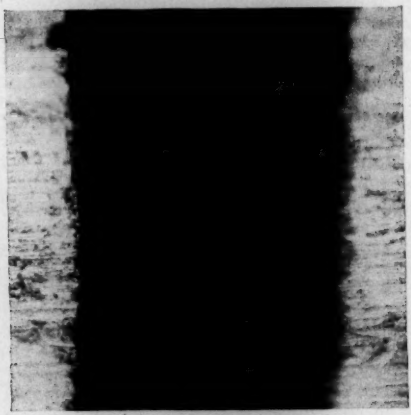


FIG. 2.

times. It will be seen that any further magnification of the scale would be futile in view of the uncertainty as to what really is the centre of the line. It is, of course, possible to rule exceedingly fine lines, but in that case the lines themselves are invisible to the unaided eye, and for many purposes this is inadmissible.

Another simple use of the microscope as a direct measuring instrument is the measurement of minute objects or distances. The simplest means of accomplishing this is to insert in the focal plane of the view

lens of the eye-piece—*i.e.*, in the plane of the real image formed by the objective—a so-called eye-piece micrometer which usually consists of a plane parallel disc of glass upon whose upper surface a fine scale has been ruled. It is immaterial what the exact value of this scale may be since the whole appliance can only be used for determining the relative sizes of the images of various objects which may successively be projected into the focal plane of the eye-piece. In practice, the scale of the eye-piece micrometer appears superposed upon the image of the object under observation, and by suitable adjustment of the object on the stage of the microscope the apparent size of the object to be measured can be read off in divisions, and, by estimation, in fractions of a division of the scale. The value of the scale must then be determined by applying to it in the same way the image of an object of accurately known size. This method of measurement has, however, considerable limitations as regards the accuracy attainable; in the first place it is by no means easy to secure such perfect superposition of image and scale that inevitable movements of the eye will produce no relative parallactic movement of the two; further, the object to be measured and the standard object of known size must be of approximately the same dimensions, and must be measured against the same divisions of the eye-piece scale, otherwise errors will arise from the effects of the variation of magnification in different parts of the field, and possibly from errors in the dividing of the eye-piece scale itself. In practice it is further by no means infrequent to find an eye-piece micrometer inserted into an eye-piece having no focusing adjustment for the view lens, and in that case it frequently requires a considerable effort of the accommodation to bring the lines of the scale into focus.

A method of microscopic measurement presenting much greater possibilities of accuracy is opened up by the use of the screw micrometer eye-piece. This appliance consists essentially of a cross-wire movable by means of a micrometer screw in the focal plane of a Ramsden eye-piece. Here, also, the difficulties of parallax have to be overcome, but on the other hand, much less delicate adjustment of the position of the object is

required, and the setting of the cross wire can be carried out with much greater accuracy than the estimation of the divisions of a scale. The accuracy ultimately obtainable depends, apart from possible inaccuracies of the micrometer screw, upon the total magnification which can be employed, and, provided that the object itself is sufficiently flat, magnifications up to twelve or fifteen hundred diameters can be used. The construction of the ordinary microscope, however, with its movable tube, is not at all suited to the use of a micrometer eye-piece with high power objectives, as a mere touch on the micrometer head is found to throw the image out of focus, and even though the focus may apparently be restored when the pressure of the hand is removed, it is by no means certain that the cross-wire will return exactly to its old position relatively to the image of the object that is being measured. In the special form of microscope which I have designed, primarily for another purpose, this difficulty is avoided owing to the fact that the tube of the instrument is in permanent, rigid connection with the limb; provided that the micrometer eye-piece itself fits firmly into the eye-piece end of the tube, the micrometer head may be freely manipulated in this instrument without disturbance either of the sharpness of the focus or of the relative position of cross-wires and image. Under these circumstances it is possible to measure extremely minute objects with very considerable accuracy.

Measurement by Projected Images.—Another method of using the microscope for purposes of measurement depends upon the use of magnified real images of the object thrown upon a screen, and either examined there or recorded by the aid of photography. This method is likely to find increasing use in the measurement of the parts of machines, and even of screw-threads requiring accurate standardisation. It appears to me that in the development of such a method there are such possibilities of a greatly extended use of the microscope for practical industrial purposes that the whole matter is worthy of careful attention in this place. For the purpose of the present account of the method I shall deal with the examination and measurement of screw-threads as a typical

example, and perhaps the most important case of the applicability of optical methods to the accurate measurement of engineering products.

If we begin by examining a small screw-thread such as that on an ordinary quarter-inch bolt, even under moderate magnification, say, with a 2-inch objective, we should probably be inclined to conclude, at first sight at all events, that no useful measurements could be made by such means; even under such moderate magnifications this object looked at from above and illuminated by reflected light gives an image, the whole of which cannot be brought into sharp focus at one time, while the perspective effect arising from the "rake," or slope of the thread, introduces further complications. Further, to attain the degree of accuracy which modern engineering requirements demand, much higher magnification than that which we have contemplated will be required. The solution of the problem, so far as screw-threads are concerned, is, I think, to be found by so placing the screw that it is viewed in profile against the brightly illuminated background of the rest of the field. If confusing and out-of-focus images of the higher parts of the thread are to be avoided the illumination must be arranged in a special way. It is obvious that for the purpose of viewing a screw-thread in profile the light must be directed into the microscope from behind the screw, *i.e.*, if the screw be placed upon the stage of an ordinary microscope it must be illuminated in the same manner as ordinary microscopic specimens that are to be viewed by transmitted light. It is found, however, both in practice and from theoretical considerations that the best effect will be obtained if the beam of light, as it falls upon the screw-thread, is as nearly as practicable parallel. If it were strictly parallel what would be seen by the observer, or would be projected on a screen, would really be a shadow or silhouettte of the screw-thread, and both the size and sharpness of the image would be unaffected by the process of focusing. It follows, therefore, that even if the beam be only approximately parallel an approach to these conditions will be obtained, and the whole of the manipulation of adjustment and focusing will thereby be rendered much easier.

Fig. 3 shows a diagram of the general arrangements indicated above for the microscopic examination of screw-threads. The magnifying power which can be used is limited to some extent by the actual dimensions of the object under examination; thus, it would obviously be impossible to examine the thread of a $\frac{1}{2}$-inch screw by means of $\frac{1}{4}$-inch lens in this way. As a matter of fact it is rarely desirable to use an objective of shorter focus than 1-inch, and for the examination of larger sizes of screw-threads the use of much longer focus objectives is necessary. The visual magnification obtainable is, therefore, decidedly limited unless one is prepared to adopt some special form of apparatus, such

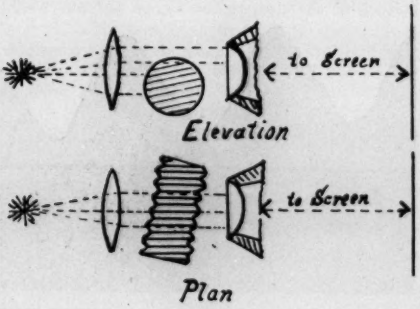


FIG. 3.

as a microscope having a tube length of several feet, or Mr. Gordon's ingenious device for the high magnification of real images. In practice, however, it is found more convenient to obtain the necessary magnification by projecting the image upon a distant screen, image distances of 6 or even 10 feet being readily available. The lantern slide, fig. 4, will serve to illustrate the crisp sharpness of image which is obtainable in this way. The image so obtained can be utilised for purposes of measurement in a variety of ways. The method that first suggests itself is to determine the magnification of the image, to measure the image itself by means of suitable scales and then to divide the numbers obtained by

the magnification in order to arrive at the true dimensions of the thread. The most serious objection to this method of procedure is that it is almost impossible to determine the magnification with sufficient accuracy, because in order to compete successfully with the best mechanical means of measurement the optical method must be rendered capable of carrying out measurements with an accuracy approximating to one hundred thousandths of an inch. Assuming that we have on the stage of our microscope, or projection apparatus, an object such as a templet or cylindrical gauge of accurately known dimensions, and that the image on the screen has a magnification of as nearly as may be 100 diameters, if the size of the image be now measured

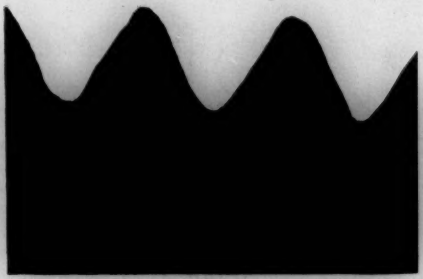


FIG. 4.

directly on a properly graduated scale it can scarcely be hoped that the actual size of the image shall be measured with an error of less than one hundredth of an inch, and since this represents an error of one ten thousandth of an inch in the dimensios of the actual object it follows that by this method, at any rate, it will be necessary to measure the image on the screen to an accuracy of one thousandth of an inch in order to reach the level of the best mechanical methods. While this might be attainable by the use of reading microscopes applied to the image on the screen, or by using a magnification of one thousand diameters, these arrangements considerably complicate the whole matter.

For many purposes, of course, measurements to an accuracy of one ten thousandth are amply sufficient, and such data as the radius of the top and bottom of the thread and the angle of the thread can be most readily and conveniently obtained by direct observation and calculation from the magnification of the image. For those measurements, however, where the highest degree of accuracy is required, it appears desirable to use the magnified image solely as an indicator, and to make the actual measurements by means of micrometer screws, moving the screw under observation. A line, or cross, drawn upon the screen then serves as an index, and the pitch and diameter of the thread can be obtained by moving it either axially or transversely until the images of the points, whose distance apart is to be determined, successsively, come into coincidence with the standard mark on the screen. Certain complications are introduced into these measurements by the fact that the axis of the screw does not lie at right angles to the optic axis of the microscope. For example, in determining the diameter of a screw the images of the thread at opposite sides of the screw are successively brought into the field, and only one of these can be in focus, since for the other the tilt of the axis of the screw will lie in the wrong direction. To overcome this difficulty, the instrument must be provided with mechanical means of rotating the screw in the plane of its axis, and about a centre coinciding with the front focus of the objective; by means of such an attachment the screw can be turned successively so as to have first the "rake" of the thread at the top and then that of the thread at the bottom in the axis of the microscope. I hope that it will be possible to adopt the optical method here outlined at the National Physical Laboratory for the purpose of measuring the threads of screws sent there for standardisation.

Quite apart from these efforts to facilitate measurement work of the highest accuracy, the optical method offers very great facilities to engineers in controlling the correct *shape* as well as the size of their products. This is particularly the case where very small objects, or parts of objects, are required to have an accurately determined size and shape. The watchmaker has long ago realised the uses of a powerful magnifying glass, and it is more than likely that engineers have over

and over again attempted to profit in their work by the indications of the compound microscope. It is, however, at least equally probable that they have not met with the success they anticipated, because the microscope was not applied in the best possible way. I may perhaps be inclined to attribute too great a virtue to an instrument that has long ago made itself indispensable to me, but I am inclined to think that there are very few branches of manufacture where a more extended use of suitably arranged microscopes would not prove of the greatest service. I hope that the method of illumination and of projection which I have described and shown as applied to screw-threads may be of service in the examination of many kindred objects in profile; with a suitable projection apparatus set up in a darkened corner of the workshop, where the workman could take his work and rapidly put it upon the stage and see its exact contour on the screen, I believe that much time would be saved in the production of objects where great accuracy is required, while for objects that on account of their size, or because they cannot be replaced if once taken out of a machine, cannot be moved to the apparatus, a portable visual apparatus could be provided. With the microscope, however, more perhaps than with any other scientific instrument, reliable and satisfactory results can only be obtained if the use of the instrument is based on sound optical and mechanical principles. For engineering purposes, therefore, special microscopes would have to be developed and their proper mode of application would need to be rendered easy and—as nearly as possible—inevitable; at the present time there is too great a tendency to think that "any old microscope" used "anyhow" will serve—and it is in consequence of failures arising from such causes that we see so few microscopes in use in the processes I have indicated. It may perhaps be urged that it will not be possible to induce workmen to use such an instrument with the necessary care and intelligence, but this assumption I am able to contradict from my own experience—an intelligent fitter, for example, will usually be both willing and able to use a reasonable form of microscope to great advantage.

I now pass on to the use of the microscope for measurements of another kind—measurements which lead us to the use of the microscope as the implement of a most im-

portant branch of physical science, viz., Petrography. The class of measurements to which I refer are those of the optical properties of transparent media available in thin laminae. Considerations of time, as well as the limitations of my own knowledge and tastes forbid me to go into very great detail on this branch of my subject.

One of the most important measurements of this class is the determination of the thickness of a thin plate of a transparent substance. If the refractive index of the substance is known the thickness is readily determined, but in most cases the measurement of thickness is required as a preliminary to the measurement of the refractive index and other optical properties. The most direct method is obviously that of setting up the thin plate on edge and measuring the thickness by one of the direct methods of microscopic measurement described above. In many cases, however, this is not possible. Thin rock sections, for instance, cannot be handled in such a way as to be set up on edge, and in any case there is always the difficulty of knowing whether the thickness at the edge observed is accurately the same as the thickness of the plate at some particular point in its area. A much more widely applicable method depends upon the use of the fine-adjustment screw of the microscope as a micrometer measuring screw, and at the outset I would warn microscopists of the great necessity of careful calibration of their screws before any attempt is made to use them for really accurate work of this kind. The actual method of measuring thickness to which I refer is one of substitution. A fine mark of some convenient kind is first made on the surface of a slip of plane parallel glass; this mark is brought into the centre of the field of view and is brought into the sharpest possible focus, an illuminating beam of wide aperture and a high eye-piece being employed, together with the shortest-focus objective available,* in order to reduce the depth of focus as much as possible. The position of the micrometer screw is now read and the objective is raised a short distance by the use of the fine adjustment only; then the plate to be measured is introduced and is

* Dry-front objectives only should be used, as the presence of immersion oil reduces the sensitiveness of the focus.

pressed into contact with the surface of the plane parallel plate below. The contact between the two plates is essential, since any distance remaining between the two will appear directly as an error in the thickness as measured by this method; if, however, both surfaces are clean and approximately flat, and a certain amount of pressure can be put upon the two in such a way as to avoid displacing or distorting the plane parallel plate, it is possible to secure practically perfect contact, the remaining space, as shown by the Newton's rings that are then formed, being exceedingly small and lying far beyond the limits of accuracy aimed at by such methods of determining thickness. The thin plate to be measured having thus been put into place, the objective is lowered towards it, again by the use of the fine adjustment alone, until the surface of the plate is seen in sharp focus. For the purpose of finding this focus it will probably be desirable to stop down the illuminating beam and to use a lower eye-piece, but the final focusing should be done with a wide aperture and a high eye-piece as before. The thickness of the plate will then be given by the difference in the readings of the divided head of the micrometer screw as registered before and after the introduction of the thin plate. The accuracy attainable with this method will depend very much upon the perfection of the instrument used, but under the most favourable conditions a thin plate of total thickness about 0.1 m/m., or, say, 1/200th of an inch, could be measured to an accuracy of one per cent.* Of course, where the thin plate can be measured directly by mechanical means, greater accuracy is attainable much more readily, but in petrological work especially instances occur where mechanical means are not available.

Actual experience of micrometer screws, as turned out by some of the best makers, has, however, convinced me that greater attention to the quality of these screws is required in the case of instruments in which the screw is

* In all work of this kind care must be taken to make proper allowance for the effect of the presence or absence of a cover-glass upon the corrections of short-focus dry objectives—the use of a compensating cover-glass of known thickness and refractive index being required for some of the measurements referred to in the text.

likely to be used for measurement purposes. Petrographers have, of course, recognised the difficulty of accurately measuring the thickness of thin plates in this way, and a device is sometimes adopted for finding the actual thickness of a rock section which depends entirely upon optical principles, although one purely mechanical assumption is involved. The device in question consists in grinding and polishing down with the rock section a specimen of a material of known and suitable optical properties. An essential feature is that the well-known substance should be one showing a certain amount of double refraction, and, as a matter of fact, slices of quartz (rock crystal) cut at right angles to the principal axis of the crystal are usually employed for this purpose. When thin slices of such a substance are examined in the microscope under crossed Nicols, they display interference colours which vary in a perfectly well-known manner with the thickness of the slice, and the colour of such a plate is thus an accurate means of determining its thickness when its exact nature and orientation in the crystal are known. This index of thickness is used in the manner already indicated on the assumption that the slice of unknown rock or mineral is of the same thickness as the slice of quartz—and it is in making this assumption that error most probably creeps into the results. Thus, if the unknown mineral or rock is either much harder or much softer than quartz, differences of thickness are very likely to arise during the grinding processes, but this can be overcome to a certain extent by the use of other minerals of known properties, such as calcite, for example; there is, however, the further difficulty that the comparison piece of known mineral can, in the nature of the case, be placed only at or near the edge of the slice of unknown material, and even if several comparison pieces are placed at various points on the edge of the specimen under investigation, it is more than likely that the surface of the finished section will differ materially from a plane, and curvature of any kind will inevitably introduce errors into the comparison we are considering. This method, like the previous one, must, therefore, be regarded as approximate only, but it can, of course, be remembered that for the identification of minerals in sections, no very high degree of accuracy in such measurements is ordinarily required.

It would occupy more time than I care to devote to the subject, to give you an account of the many measurements which are made under the microscope for the purpose of differentiating minerals from one another according to their optical behaviour, chiefly in regard to the polarisation of light. Beautiful and interesting as these are, I must leave their treatment to some one who could deal with them on the basis of a much wider experience than I have had, and I will leave this aspect of microscopy after dealing very briefly with the simplest and perhaps most widely applicable optical measurement that can be made under the microscope, viz., the measurement of the refractive index of substances available only in thin plates or in small quantities. Perhaps the simplest measurement of this kind is made by the method of direct comparison. Just as it is possible to determine the density or specific gravity of a body—even if it be available in minute fragments only, by immersing the body or fragments thereof in a series of liquids of graduated density, and thus finding in which liquid the body floats and in which it just sinks, and thus concluding that the density of the body must be intermediate between that of these two liquids, so the refractive index of minute fragments of transparent bodies can be determined by observing their relative visibility when immersed in a series of fluids of increasing refractive index. The visibility of a body when immersed in a fluid, apart from differences of colour and transparency in the body itself, depends entirely upon the difference of refractive index between the body and the fluid. When a colourless transparent body, such as a fragment of good glass, is immersed in a colourless, transparent liquid of the same refractive index, the fragments of the solid become almost entirely invisible; the rays of light undergo no deviation on passing from the liquid into the solid, or *vice-versâ*, and thus pass through the body unchanged in direction, while little or no light is, under these circumstances, reflected from the surface bounding the two media. On the other hand, when there is a decided difference of refractive index between the solid and the liquid in which it is immersed, the rays of light undergo deviation as they pass from liquid to solid, and *vice-versâ*, and the body becomes visible by the distorting effect of the irregular surface, and also by the considerable scattering of light

due to reflection at the interfaces of liquid and solid.

There are also the phenomena of total reflection to be considered; total reflection is liable to occur only where light passes from a more highly refracting medium into a less highly refracting one, and this will occur as the light *enters* the solid body, or as it *leaves* the solid and enters the liquid, according as the refractive index of the solid is lower or higher than that of the liquid; if the character of the reflections in such a case are carefully observed and considered, it is easy to decide whether the refractive index of the unknown body is higher or lower than that of the known liquid. It should, of course, be noted that this method may be reversed and fragments of a body of known refractive index utilised to determine the index of an unknown liquid—and this application might perhaps find some practical use in certain industries where solutions of definite density are required—the visibility or invisibility of an immersed sphere of glass of the proper refractive index serving as a ready guide for such a purpose.

The method, however, has certain limitations. In the first place it is not readily applied to coloured substances, at all events if the colour is deep enough to be visible in minute fragments. Further, liquids of great ranges of density, although more or less available, require a good deal of care in storage and manipulation, otherwise they are liable to undergo changes of composition or of concentration, while, finally, the refractive index of all liquids is subject to a very considerable temperature co-efficient, although this last error may not greatly affect the results to the degree of approximation attainable by such methods. A more serious drawback is that the method cannot be applied to minerals forming part of a rock-section without breaking the section up into powder. For the latter class of objects, however, another microscopical method is available, provided that the thickness of the section is known. This is the well-known method of comparing the "optical," or apparent thickness of the plate with its real thickness. When an object or mark is viewed through a thickness d of a material having a refractive index n, assuming a normal line of vision and plane parallel surfaces for the plate under observation, the mark is seen at a distance d' below the upper surface of the plate, which is given by the formula

$d' = d/n_D$. Since d is supposed known, and d' can be observed by focusing first upon the upper surface of the plate and then on a mark on the lower surface, and observing the movement of the micrometer screw of the microscope in making this change of focus, the value of n_D can be calculated. The accuracy attainable is limited by that with which the true thickness of the plate is known, and also by the accuracy of the micrometer screw, but under favourable conditions I have been able to measure the refractive index of a thin sheet of glass in this way with an accuracy of about $\frac{1}{3}$ per cent., *i.e.*, in an index of 1.500 the error would not exceed .005, but in that case the thickness of the glass was measured mechanically.

Microscopic Study of Metals.—I now pass on, with some sense of relief—to myself, at all events—to my own special branch of microscopy, viz., to the application of the microscope to the study of metals. Here I shall probably be tempted to dwell too lovingly upon details that could be of little interest to those concerned with it as a branch of optics, but I will endeavour to confine myself to the optical aspect of the subject. The whole science of metallography is not very old, but it has already suffered from both a boom and a consequent period of depression from which it has not yet fully emerged. While I fully agree with those who deprecate the extravagant claims that have been rashly advanced for it, I would like to record my conviction that we have as yet only skimmed the surface of the subject, and that the microscope—used with full knowledge and skill by men who do not merely regard it as a glorified magnifying glass—is yet destined to open up entirely new territories of knowledge in this field—and knowledge, too, that will ultimately find the widest practical application. At the present moment, the science of metallography, of which the microscopic study of metals is but one part, stands as the link between physical chemistry and metallurgy, and is to be regarded—in my opinion, at all events—as being of vastly greater importance as a method of research—that is, as a means of acquiring new and valuable knowledge—than as a method of routine testing, although even in that direction many useful applications are to be found. From the Optician's

point of view this branch of microscopy is important, since it has already opened up a large field for the use of the microscope, and, consequently, for the *sale* of these instruments, while it has already led to the development of new types of microscope.

For the purposes of microscopic examination, the surface of a piece of metal is first of all prepared so as to present as smooth and featureless a surface as possible. Ordinarily this is accomplished, to a variable degree of success, by various methods of polishing. Quite a special technique has grown up in this direction, and although a good many fruitless attempts might have been saved if metallurgists had studied the methods of grinding and polishing used by Opticians, yet at the present time I think it possible that the Optician might learn something from the metallurgist. It is at least a remarkable fact that the true nature of the polishing process proper was first definitely recognised by men who worked with metals. This is a subject of very great interest, but I must not dwell upon it here, although it would be well worthy of close consideration by this Society.

The only point to be noted in the present connection is that for purposes of microscopic examination, a featureless polished surface, although an essential preliminary, is in itself useless. Various methods are, therefore, used for the purpose of producing upon this surface a pattern which shall be clearly visible under suitable magnification, and which shall bear some definite relation to the internal structure of the metal. It is not, of course, self-evident that it is possible to produce such a pattern at all, but that possibility has been demonstrated beyond all doubt by the accumulated results of experiments made by many workers. Broadly speaking, metals may be divided into two classes, viz., those having a duplex structure consisting of two chemically distinct bodies, and those which consist entirely of one chemically homogeneous material; the former class embraces many alloys, while all pure metals belong to the latter class. In metals of the former class, the production of a characteristic surface pattern is comparatively easy; in nearly every case one of the constituents of the metal is more readily attacked by chemical agents than the other, and if such

an agent—as, for example, a dilute acid—is applied to the polished surface, those parts of the surface which consist of the less resistant material will be attacked and partially dissolved, while the more resistant areas remain untouched and retain their original featureless smoothness. The effect is, first of all, a definite "etching" of the surface into hollows or depressions representing the corroded material. Sometimes these corroded areas are further differentiated by a thin coating of a coloured substance which has been formed, or separated, during the solution of the one constituent, but such coatings are apt to be very irregular in character, and generally adhere to the surface so feebly that even gentle wiping of the etched surface suffices to remove them either wholly or in part. The greatest reliance must, therefore, be placed upon the differences of level and of smoothness that arise in consequence of the process of differential etching just described.

It is interesting to consider exactly how such surface irregularities, apart from any differences of colour, give rise to well-defined microscopic images. The nature of the image produced depends entirely upon the manner in which the surface is illuminated. It is, of course, evident that a specimen of metal, only one surface of which has been prepared, cannot be examined by any other than *reflected* light, but it is possible to arrange that the light incident upon the surface shall fall upon it either normally or obliquely. When normal illumination is desired, the light must be sent down upon the specimen along the axis of the microscope, while for oblique illumination the light is directed obliquely upon the surface from some direction, making a greater or less angle with the axis of the microscope. The appearance of objects under "normal" illumination is not at once realised by those unaccustomed to its effects, since it is a kind of lighting which we rarely experience when examining objects with the unaided eye—under those circumstances the light would have to emanate from the eye itself, and we rarely use the mirrors of a retinoscope for illuminating the objects we are examining.

In metallic specimens we have the further feature to consider that the surfaces, although not perfectly specular in character, are yet good metallic reflectors.

The diagram, fig. 5, will, however, at once make the whole matter clear; the lines marked with downward-pointing arrows (*i*, *i*, *i*) represent the direction of incident light, AA is the aperture of the objective, while SS is the surface of the specimen, *r r r* are the directions of reflected rays. It will be seen at once that all those parts of the surface which are sufficiently nearly horizontal in the diagram to reflect the light back into the aperture of the objective will appear bright under this illumination, while all those parts of the surface which are sufficiently inclined to the axis of the microscope will throw the light falling upon them outside the aperture of the objective, and will, therefore, appear dark in the field of view of the instrument. If we apply

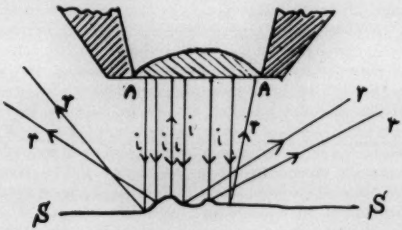


FIG. 5.

this principle in the first place to the etched surface of the duplex alloy already considered, we see that obviously the untouched portions of the surface, representing the areas occupied by the more resistant constituent, will appear bright. The surfaces of the corroded areas representing the other constituent, however, will not in general lie parallel to the original surface—the etching action eats out the surface into minute pits and hollows, and the sides and slopes of these irregularities will, as a rule, scatter the light to a considerable extent, and thus send the greater part of it outside the aperture of the instrument; these corroded areas, therefore, appear relatively dark under "normal" illumination, especially when the etching process has been carried to a considerable depth.

The diagram of fig. 5 may also be used to explain the fact that when oblique illumination is applied, in place of normal, some of those areas which appeared dark under the latter mode of lighting will appear bright, while all those that appeared bright under "normal" light will appear dark under oblique illumination. The reason is simply that rays striking the surface in the direction RR will be reflected into the aperture of the instrument only if they strike a surface, or element of a surface inclined at a suitable angle, while the horizontal surfaces such as *s s* will reflect the light entirely away from the microscope. If the oblique light be allowed to fall upon the surface not in one plane only but in all directions, the image seen in the field of view will be accurately the negative of that seen under "normal" illumination, since in that case *all* inclined faces, or facets, will appear bright, while all horizontal surfaces will appear dark. This form of illumination is, of course, obviously impossible, although the well-known Sorby parabolic reflector gives a partial approach to it; the use of a parallel, or nearly parallel, beam of oblique light, whose direction and plane of incidence can be varied, is, however, a much more instructive mode of illumination since it gives a better clue to the orientation of the different reflecting facets of the specimen. This question of orientation is not, perhaps, of very great importance in connection with our present methods of examining duplex alloys, but when we come to investigate the structure of chemically homogeneous metals, the reverse is the case. Taking the simplest case of a pure metal, it is obvious that any etching reagent that is applied to the polished surface can find no chemically different and less resistant constituent to act upon preferentially, and, as a matter of fact, the action that takes place is of a totally different character.

In order to understand what takes place we must realise—what is now fully established—that in a pure metal, at all events, we are dealing with an aggregate of minute crystals. These crystals have grown during the first solidification of the metal from the molten condition, and in their growth each has interfered with, and limited, the development of its neighbours; such crystals, therefore, do not exhibit the regular geometrical outlines found in crys-

tals that have been formed freely, but the metallic crystals, nevertheless, possess the true internal structure of crystals. The essential feature of this structure is the fact that within the boundaries of any one crystal all the minute particles or elements—"crystal bricks" one might call them—of which that crystal is built up, are similar in shape and orientation, just as the bricks in a well-piled stack are all alike and parallel with one another. But this similarity of orientation is confined to the members of the same crystal; in adjacent crystals the orientation is different; it is, in fact, this difference of orientation which distinguishes one crystal from another. For our present purpose the important result is that a surface cut at haphazard through an aggregate of such crystals, will be made up of areas, more or less polygonal in outline, representing the parts of the surface lying within individual crystals, and within each of these areas the molecules, or groups of molecules, will be arranged on a definite plan, but this plan, or orientation, will change in direction, though not in general character, from one area to another. When such a surface is exposed to the action of an etching agent what occurs is this:—The surface layer, or film of smoothed-out débris of crystal fragments, formed during grinding and polishing, is first of all dissolved, and the actual surfaces of the crystals are exposed; these are now attacked, and the attack takes the form of a gradual unbuilding of the crystal structure, and the rate at which this goes on varies from crystal to crystal, according to the direction, relative to its particular orientation, in which the crystal happens to have been cut. Thus, the final effect of etching in such a case is to produce upon the surface of the metal a number of more or less polygonal areas at varying levels, these various levels being connected by short and somewhat irregular slopes, while each area—under favourable conditions—is etched out by the removal of layers of "crystal bricks" into a congeries of minute brick-faces, or facets, the orientation, or "lie," of these facets being different from one crystal area to the next. What is the resultant appearance under the microscope? Under normal illumination we may, in pursuance of fig. 5, expect to find that the short slopes denoting the boundaries of crystals will appear as black lines or

bands outlining the crystal areas. Further, if the etching has been carried rather far, we may expect to find differences in brightness between various crystal areas, according as the faces of the etched-out facets are more or less inclined to the horizontal, while under sufficient magnification we might even expect to see the actual facets themselves. All this is in exact accordance with observed facts, and as a matter of fact it was the correct interpretation of these and allied microscopical features that first led to the recognition of the truly crystalline character of metals. Perhaps the most striking of all these microscopic appearances is the view presented by an etched specimen of such a metal when viewed under oblique illumination by means of a beam of moderate angle. In that case the etched-out facets of some of the crystals are so placed as to reflect the light into the microscope, and the entire surface of the crystal in question shines out brilliantly, while other crystals are quite dark, and others, again, appear less brightly illuminated. If the direction of the incident light be altered, as for instance, by rotating the specimen on the stage of the microscope, the facets of other crystals come into the favourable position, and thus new crystals shine out while others are eclipsed. This play of light is an exceedingly beautiful phenomenon, which is quite characteristic of an etched crystalline surface —so much so, indeed, that the appearance has received the name of "oriented lustre." I hope that it will be possible to see this phenomenon on the screen, but in any case some good examples are shown under some of the microscopes on the table.

My object in going into such detail in connection with these matters is, apart from the beauty and interest of the phenomena themselves, to draw attention to the fact that a correct understanding of the physical facts underlying what is seen in the microscope, is dependent upon a knowledge of the optics of illumination and of image formation. I have, of course, here only given the most elementary part of the theory of the subject, but will revert to it again when I come to deal with the instrumental means used for obtaining the various effects described. Before I turn to that aspect of the subject, however, I wish to give yet another example of the

methods of interpreting the microscopic appearances of opaque surfaces viewed by reflected light.

In 1898, when I had the privilege of working as a research student with Professor Ewing in his laboratory in Cambridge, we wished to study the behaviour of metals while undergoing plastic deformation. For that purpose we polished for examination part of a strip of thin sheet-iron, and placed the strip in a small stretching machine, which was screwed on to the stage of the microscope, an example of which is on the table. The specimen having been etched, presented the usual appearance of polygonal crystalline outlines, but the etching was not carried far enough to bring out any visible detail within these polygonal areas. We next proceeded to strain the specimen by turning the thumb-screw attached to the machine, meanwhile watching the metal under the microscope. To our amazement we observed that the clear white faces of the crystal polygons began to be cross-hatched by dark lines, forking and branching all over the surface, but showing a tendency to remain roughly parallel within each crystal. Then came the question to be solved—what were these dark lines? At first sight they gave the impression of being simply very minute cracks, and, indeed, it was as "cracks" that we first spoke of them. But a little consideration showed that, since the strength of the metal was not impaired by the amount of strain we had applied, the metal could not be thus riddled with fissures. What is now, I think, proved beyond doubt to be the true explanation then occurred to us, viz., that these lines were due to the formation of minute steps in the surface of the metal. These steps are caused by a sliding or slipping action which goes on within the crystals, which adjust themselves to the strain by the sliding over one another of layers of the minute crystal elements or "bricks," of which each crystal is built up. This sliding takes place upon cleavage or gliding planes in each crystal, and these are, of course, differently oriented in each crystal. Upon the basis of this explanation, together with much evidence from other sources, we were able to arrive at a somewhat general theory as to the manner in which metals undergo plastic deformation. The entire theory, however, rested upon the proof that the black lines just described are really of the nature

of steps on the surface of the metal. The proof which Professor Ewing and I gave at the time was based upon purely optical principles of interpretation, and it is for that reason that I have introduced the matter here. Fig. 6 is practically a repetition of fig. 5, but a stepped surface is here sketched as seen under normal and oblique illumination. When the rays of light fall normally upon such a surface, the flat spaces between the steps will appear white, while the short sloping surfaces of the steps themselves will throw the light outside the aperture of the objective and will, therefore, appear dark; but when the light falls upon the surface

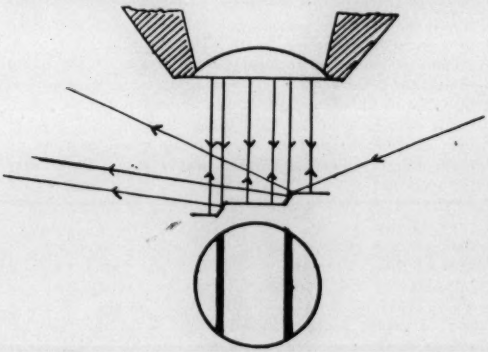


FIG. 6.

obliquely from the direction which the slopes of the steps are facing, then the short sloped surfaces will reflect light into the microscope and will appear as bright lines against the background of the flat areas, which will now appear dark. I have upon the table an example of this kind, with arrangements for changing rapidly from normal to oblique illumination, and also for rotating the specimen so as to bring different sets of these steps into the position for catching the light, and a moment's examination will convince you that the actual phenomena correspond to the indications of this theory.

So convincing, indeed, is this demonstration, that it

has always been surprising to me to find that that it did not at once meet with complete acceptance, but certain eminent French metallurgists at first failed to appreciate the full validity of this argument, and this has led me to devise certain further methods—some of them, perhaps, of optical interest—in proof of my views. The view of the French metallurgists was, at all events at one time, that these lines were due to the formation of ridges or grooves on the surface of strained metal—the formation of such a configuration being ascribed to a general "rumpling" of the metal under the strain. Fig. 7 is a sketch of the behaviour of such a surface under normal and oblique light; as you will readily see by applying

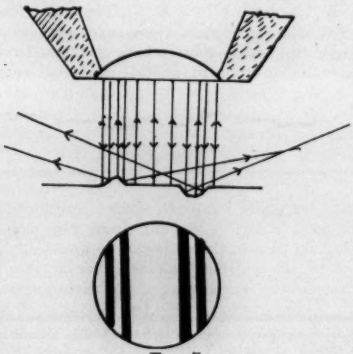


Fig. 7.

the reasoning indicated above, such a surface would show dark lines, although they should theoretically be double, with a bright line between. It might, however, be argued that if the ridges were small enough the two lines might merge, or be confused, owing to diffraction phenomena. But it will also be evident that if such a surface be illuminated by oblique light so directed as to cause one set of lines to appear bright on a dark background, the oblique-light appearance of the stepped surface will be exactly simulated; there is, however, an important difference. In the case of the stepped

surface, if the specimen be rotated through 180 degrees, or if the oblique light be directed from the opposite side to that originally used, the lines will no longer appear bright but will remain completely invisible, but if this be done in the case of a ridged or grooved surface the lines will be equally bright in either position. Now examination under oblique light of moderate intensity shows that the actual appearance corresponds to the behaviour of steps and not of ridges or furrows, but it was found that if a very intense light be employed, then some lines are just faintly visible in the second position also. What I think is the true explanation of these facts is best illustrated in the case of a metal in which these lines are developed on a much larger scale than in iron, viz., in lead. Here it is frequently found that apparently the same set of lines light up when illuminated from two opposite directions. It has, however, occurred to me to enquire whether the lines seen in the

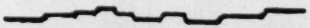


FIG. 8.

two cases are really identical, since a configuration such as that seen in fig. 8, arising from two sets of steps occurring on planes whose line of intersection is nearly parallel to the polished surface, might give rise to somewhat similar appearances. To distinguish between the two configurations, *i.e.*, two systems of steps or ridges and grooves, an optical method of interpretation is again available. Suppose the surface to be simultaneously illuminated from both sides, using blue light on one side and red light on the other. If we are dealing with a series of ridges or grooves we should see as many blue lines as red ones, since every up-slope of a ridge must have a corresponding down-slope; if, however, we are dealing with two independent systems of parallel, or nearly parallel, steps, there is no necessary connection between the number of red and of blue lines that will be seen. I show a photo-micrograph in natural colours on the screen illustrating the actual appearance. This shows, quite unmistakably, that the distribution of red and blue lines, *i.e.*, of "up" and "down" slopes,

is quite arbitrary, and that they are certainly not equal in number.

Finally, in order to show that the conclusions reached by arguments of this kind are valid and reliable, I will show on the screen a photo-micrograph, taken under a magnification of 2,000 diameters, in which an actual section of such a surface of strained metal is shown. This has been obtained by the aid of an embedding process, borrowed partly from the biologist, who embeds his delicate organic structures in paraffin-wax to protect them during sectioning, and partly from the optician who beds his glass in pitch, or edges it with other glass if he wishes to grind and polish a sharp edge. The method of embedding, which I have adopted for this purpose, consists in depositing a thick layer of copper on the surface whose section is to be examined—the deposition of the copper being carried out by electrolytic (galvanoplastic) means. This copper is left on the surface and the section is cut through the compound mass, the presence of the copper serving to protect the delicate edge of the section. As you will see by the example shown, this method yields extremely sharp outlines, and conclusively demonstrates the fact that the surface of strained metal is really stepped in the way that optical considerations had originally led Professor Ewing and myself to conclude.

Having now endeavoured, by means of examples, to show the manner in which optical considerations must be employed for the interpretation of microscopic images of opaque bodies and of metallic surfaces in particular, I will devote a short space to the instrumental means required for such work. Again I must begin by saying that for the man who merely wants to use the microscope as a rather more powerful kind of magnifying glass, making no attempt to realise the physical character of the object whose image he is examining, practically any reasonable form of microscope is "good enough"; but I must also reiterate the opinion that the future of metallography does not lie in the hands of such men, while already much disappointment has been caused owing to the fact that men expected to see too much without making the necesary effort to understand what the microscope could, and could not, do for them. I should perhaps warn you that I occupy an extreme posi-

tion among metallurgists in this respect, but I confidently leave it to the future to decide whether the direction I am indicating does not lead to the most fruitful field of microscopic study as applied to metals. At all events, a better understanding of the optics of the subject would undoubtedly save us from some of those smudgy so-called "photo-micrographs," which appear as "illustration," to some metallurgical papers.

With this digression I return to the subject of the microscopic equipment required for metallurgical work of the best kind. Apart from the objectives and eye-pieces, some form of tube for holding these, and a firm stage for holding the specimen of metal, the first essential is a means of illuminating the specimen by either "normal" or oblique light. "Normal" illumination is nearly always obtained by sending a beam of light down the tube of the microscope axially, and allowing the objective to act as a condenser, concentrating the beam on the spot under examination. For this purpose the beam of light enters the tube of the microscope at one side and is reflected downwards to the objective either by a sheet of very thin glass placed at an angle of 45° to the path of the beam and the axis of the microscope, or by an opaque reflector, such as a totally reflecting prism or a piece of silvered glass, so placed as to cover a portion of the objective. The first method, *i.e.*, the use of the thin glass reflector, is obviously wasteful of light, since the glass disc reflects only from 15 to 20 per cent. of the light falling upon it; taking the higher and more favourable figure, and assuming, for the purpose of comparison only, that the *whole* of the light falling upon the back of the objective is first transmitted down to the specimen, there reflected, and then transmitted, up through the objective again without any loss or absorption at all, the beam leaving the objective on its way to the eye-piece will only contain 20 per cent. of the original light, and of this a further 20 per cent. will be sent outwards by the reflecting action of the glass disc—the beam that passes on to the eye-piece thus having a relative strength of only 16 per cent. of that of the original beam. It appears at once that if we could so increase the reflecting power of the disc that it should reflect 50 per cent. of the light falling upon it at 45°, and transmit the other 50 per cent., we should get

a better result—the beam passing to the eye-piece, under the same assumptions as above—with a relative strength of 25 per cent. This is not easily realised in the case of a "transparent" reflector covering the whole aperture of the objective, but it can be approximately realised by removing part of the glass disc and by silvering the remaining part. Such an arrangement has the further advantage that the light forming the images does not pass through a thin sheet of glass in its passage up the tube; the presence of such a thin sheet should, theoretically, make very little difference to the resulting images, but in practice such thin sheets are never flat or parallel, since they are merely cut from blown sheets and are not optically worked; consequently, there is always a very decided deterioration of the image when the glass-disc reflector is used. On the other hand, the clear glass reflector is the only means of obtaining strictly normal illumination of our surfaces, since by its means alone can a truly axial beam be passed down the microscope. For certain purposes of observation, therefore, the glass disc remains essential, but it is not convenient, or desirable, for other purposes. A very close approximation to truly axial illumination can be obtained by the use of opaque reflectors, but for this purpose it is essential that the precise position, both vertically and horizontally, of the reflector itself should be readily regulated, as well as its angle to the incoming beam of light. Now, the ordinary form of "vertical illuminator," as sold with most metallurgical microscopes, does not provide for these adjustments, and this appliance has the further disadvantage of being a detachable fitting, which is interposed between microscope tube and objective—just at that point in the microscope where the greatest rigidity and directness of attachment are required. Both of these objections are completely met by the arrangement of the instrument which I have designed for this work, and examples of which are on the table. I do not propose to describe this instrument in detail here, since a full account of it has already appeared, so that I will merely draw attention to those features which are connected with the question of the interpretation of images. Among these the adjustments of the illuminator are probably the most important, since they enable the operator to alter the lighting from

truly axial to very slightly oblique by the interchange of one form of reflector for another without affecting the focus-setting of the most powerful lenses. This enables the operator to make accurate comparisons between appearances under slightly different lighting which would be impossible with the older forms of illuminator.

In addition, by utilising the wide angular apertures of high-power objectives, it is possible to send oblique beams upon specimens while examining them under immersion objectives, where the introduction of oblique light has hitherto been impossible. An example of oblique illumination under an immersion objective obtained in this way is shown on the table.

A further point of some optical interest arises in connection with these illuminators, and it serves to illustrate the necessity for having a metallurgical microscope provided with a completely rotating stage. When an opaque reflector is used, the closing of half the aperture of the objective, which is covered by the reflector, affects the formation of the image. The resolving power of the objective, in fact, is considerably smaller in that direction in which the aperture is only one-half of its full value. Now, in alloys we have frequently to deal with finely laminated structures, and sometimes the question as to whether a given structure is visibly laminated or is "amorphous" or non-descript in structure affects the conclusions to be arrived at as to the past history and properties of the metal. Now, I have frequently observed that when a specimen showing fine lamination which can just be resolved in the most favourable position is turned round through a right angle, the laminations disappear and an indefinite grey or mottled appearance is seen. This observation shows not only the importance of using lenses of the highest resolving power obtainable, but also the need of examining a specimen in all positions by the aid of a rotating stage. This question of resolving power which I have just touched upon is, I believe, at the bottom of a good deal of the controversy which has raged around certain questions in connection with the micro-structure of steel. Some of the more minute structures which result from rapid cooling are undoubtedly at, or just beyond, the limits of resolution by means of the microscope—and the opinion formed as to the existence or non-

existence of a certain constituent or structure thus becomes a question of the optical aids at the command of various observers. In this connection I base great hopes on the fruits of applying the "ultra-violet microscope" to the study of metals. An account of this apparatus, as used for transparent objects, has, I believe, been given to this Society, and I am happy to say that an outfit of this kind is now being installed at the National Physical Laboratory, and the doubling of the ultimate resolving power, which is promised by the makers of this apparatus, will throw fresh and valuable light on the most minute structures found in metals.

Limits of time and space prevent me from carrying this discussion of the optics of metallography any further in this place, but I trust I have said enough to show that this aspect of metallography is worthy of much greater attention than it has yet received. Before leaving this part of my subject I will, however, emphasise what is really quite obvious: if the science of metallography can be advanced by the aid of improved optical appliances, this advance can only be effected by the close co-operation of metallurgist and optician. As in all new developments, both parties must spend time, energy and money if development is to take place; the metallurgist will, I think, be quite ready to do his share, and it will be well, even from the purely business point of view, for the optician to do his best also. At first, this development of a new science may lead to little new business for the optician, but if the science can be advanced practical applications of its methods can, and will, be found, and these will penetrate to every works and engineering workshop of any size in this country and throughout the world, so that a really great demand for the right instruments will be created; those who have aided the evolution of the instruments will be best situated to reap the reward of their foresight. This is by no means a visionary forecast, for we have seen the same thing occur in other industries, as witness, for example, the rapidly spreading use of electrical pyrometers at the present time; those who put their brains and money into the development of these instruments showed their scientific foresight, and that is now translating itself into business profits.

Micro-Chemistry.—In the remaining section of this address I wish to deal, very briefly, with one more application of the microscope, and that is, to the purposes of "micro-chemistry." This is a subject which has, I think, a considerable future before it, but it has received little or no attention in England. The subject has been developed almost entirely by (the late) Professor Behrens, of Leyden, and he has carried it to a very considerable length in his books on "Micro-Chemical Analysis," "The Technique of Micro-Chemistry," and others, while several other works, including a very beautiful atlas of the subject, have been published in German. I am not certain whether any of these books have even been translated into English, and I have certainly never seen the method in use in any English laboratory. The *raison d'être* of the whole subject is this, that it is frequently desirable to determine the chemical composition of substances which are only available in minute quantities, such as minute fragments of minerals found in rocks, enclosed impurities in a metal, and so on, through the whole range of materials which are of sufficient interest, or value, to repay close investigation. Now, the whole body of ordinary chemical methods of analysis, either quantitative or qualitative, necessarily breaks down when the material is only available in very minute quantities. A skilled analyst may be able to arrive at some estimate of the composition of a substance when one tenth of a gramme is all that is available, and some information may even be gleaned from a quantity like one-hundredth of a gramme—but that is quite the lowest limit, and long before that is approached the usual apparatus and methods of chemical analysis require much modification. This modification has been made in a radical manner by Behrens in the introduction of microscopic methods. In ordinary chemical tests, the recognition of a given constituent is achieved by observing whether or not, under given conditions, a certain reaction takes place—such as the evolution of a gas or the deposition of a precipitate of a particular character. In many cases these tests are exceedingly delicate, but for the convenient manipulation fairly large quantities of solutions are required, since the mixing of the reacting substances is generally done in test-tubes or beakers of moderate size. In Behrens'

method single drops of the reacting liquids are mixed on an ordinary glass slip and the resulting reaction is watched under moderate magnification. The kind of reaction which is most readily recognised is the formation of crystals of a sparingly soluble substance, more especially when these crystals are strikingly characteristic in shape or colour. In some cases, the crystals form almost as soon as the two drops are mixed, while in others their formation only sets in when evaporation has reduced the bulk of the liquid very greatly. Examples of these test reactions are on the table. To give you some idea of the delicacy of these tests, I may mention that it is quite possible by this method to recognise with certainty the presence in a single particle of dust or powder a quantity of sulphur, for instance, whose actual weight would only be 0.00019 milligrammes, *i.e.*, about two ten-millionths of a gramme. This is certainly a notable achievement and well worthy of full consideration by those who have to deal with the detection of minute traces of substances. Here, also, the optician and the instrument maker have a wide field before them. Special microscopes for such work have been produced by continental makers, the principal feature being the provision of appliances for warming the stage to any desired extent. Actual use of some of these instruments has, however, convinced me that much remains to be done in the evolution of a desirable form of chemical microscope, while more still remains to be done in endeavouring to arouse an interest in these methods in the proper quarters. It is with that object in view that I have mentioned the subject to-night, and have shown you some examples of this beautiful method of analysis, and I hope to take further steps in the same direction at a later date.

I have now endeavoured to touch upon a large number of possible applications of the microscope, in a manner that has been intended to suggest ideas rather than to deal exhaustively with any branch of my subject. My object I have stated at the outset to be to illustrate the physical as distinct from the biological uses of the microscope. I lay no claim to have exhausted these uses, nor do I pretend to adjudge the relative values of these various uses of the instrument, but I hope that I have shown enough to prove that the observation of the ex-

ceedingly minute is of no less interest and value in the physical than it has proved to be in the biological world. In the abstract this may be readily enough admitted, but if we compare the number of microscopes sold—or in use—for biological purposes with those used for physical work we shall find an enormous preponderance of the former. In the interests of education, of all the industries dependent upon metals and the measurement and observation of metals and—last, but by no means least—in the interests of the optical industry, it is eminently desirable that the balance should be redressed; that the engineer, the physicist, the metallurgist and the chemist, should come to look to the indications of the microscope as habitually as does the biologist, the bacteriologist, the brewer, and the pathologist, and, finally, that all users of the microscope should wield the instrument in that intelligent and scientific manner which can alone be acquired by a full understanding of the optical principles underlying its performance. In some small measure to promote these ends has been my endeavour this evening.

THE TESTING OF TRIAL CASES.

By F. J. Selby.

Read before the Optical Society on May 16th, 1907.

The question of the verification and certification of the "trial cases" of lenses used by oculists has been for some time under consideration by a Sub-Committee of the Optical Standards Committee of this Society. The preliminary work necessary with a view to arranging for such certification has been in progress at the National Physical Laboratory, and I hope that a short account of this work, and a discussion of some of the considerations which necessarily arise in connection with it, may be of interest. I do not, of course, propose to enter upon any questions which are still under the consideration of the Committee, and which have to be decided by them; but there are certain matters of fact upon which the information available is not perhaps so definite as

Fig. 1.

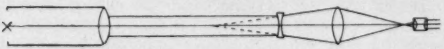


Fig. 2.

it might be, and I think that the particulars which may be furnished in a discussion of these matters by the members of this Society may be of general interest.

In June, 1900, Dr. Drysdale read a paper before this Society in which he described an apparatus for the rapid determination of the powers of spherical and cylindrical lenses. The optical arrangement of this is shown diagrammatically in figs. 1 and 2. Shortly, the apparatus consists of an optical bench some two meters long, with a collimator at one end and

a telescope at the other. The telescope is not adjusted for infinity, but is focused for a point at about the middle of the bench: it might perhaps be described as an auxiliary lens—the object glass—and a microscope —the eye-piece. In testing a lens the focus of this telescope is not altered; the lens is moved so that first its surface comes into focus, and then until the image, whether real or virtual, of the collimator cross-wires is in focus. The distance through which the lens is moved between these points is its focal length, provided the thickness be negligible. For thick lenses a means of rotation about a vertical axis is provided, and the lens can be adjusted so as to get the back nodal point over the axis of rotation.

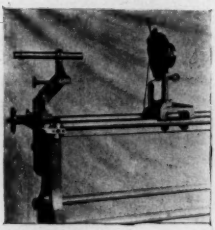


FIG. 3.

FIG. 4.

If the lens under test be of positive power it forms a real image of the collimator cross-wires, and of this image a second real image is formed by the object glass of the telescope, or what I have called the auxiliary lens; the latter image being viewed by the microscope eye-piece. The first real image might equally well be viewed by the microscope direct, without otherwise affecting the measurements. This would have the advantage that a greater range on the bench could be employed over which the positive lens could be moved, so that lenses of longer focal length could be directly dealt with.

If the lens under test be negative the image it forms of the collimator cross-wires is virtual, and the auxiliary lens is necessary to form a real image, to be viewed by the eye-piece or microscope.

The modification of Dr. Drysdale's arrangement thus indicated is the one which has been adopted at the National Physical Laboratory for the testing of trial case lenses. The method was suggested by Dr. Glazebrook, and the bench employed was constructed by Messrs. R. and J. Beck. For positive lenses a collimator and observing microscope alone are required. We prefer, however, to employ the principle of auto-collimation; this gives much better results at high powers, and is preferable to work with throughout, as it avoids variation in the magnification.

For negative lenses the arrangement is identical with that already shewn in Dr. Drysdale's apparatus (fig. 2). In this case auto-collimation cannot be employed as the field of view obtained is too small for convenient working. The difficulty as to magnification is, however, met by a suitable adjustment of the auxiliary lens. For illumination we use a mercury lamp, cutting out the violet rays; practically, this is equivalent to using sodium light, the yellow mercury lines differing only very slightly in their position in the spectrum from the sodium lines. In our ordinary working we have allowed the green lines to remain; for work of the highest accuracy these also would have to be eliminated.

I do not propose to-night to go into detail as to the design of the bench, which has been described already in the Catalogue of the Optical Convention. Figs. 3, 4, and 5 show some of the arrangements. But I must say a few words as to what can be done with it.

In testing spherical lenses we have to determine the power and the centering error. In the case of cylindrical lenses the error in the position of the axis has also to be found. The bench is designed so as to enable all these measurements to be made as rapidly as possible. As already explained, in the determination of the focus the lens only is moved; the amount of its motion gives the focal length required. This motion can be given to the lens from the observing end of the bench, and the readings are also taken without the observer having to change his position. In order to determine the centering error, the lens holder is made to rotate about an axis parallel to the length of the bench, and this rotation can be given to the lens from the observing end; from the shift of the image the centering error is ob-

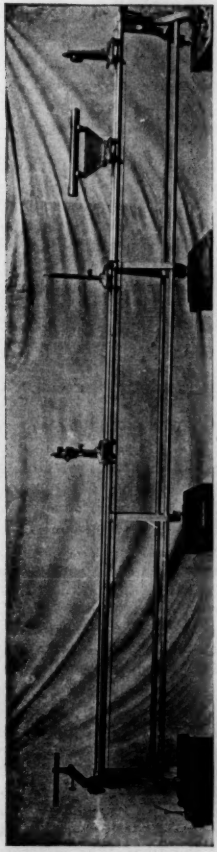


Fig. 5.

tained. The same rotation enables the axis error to be observed in the case of cylindrical lenses, the rotating head of the lens-holder being provided with a divided circle.

I have already mentioned that in dealing with thick lenses the holder on Dr. Drysdale's bench can be rotated about a vertical axis to determine the position of the nodal point. In view of the other motions necessary on our bench it would not perhaps be very easy to provide this additional rotation. But for our purposes this rotation can be dispensed with. The thick lenses of the same power in different trial cases differ only very slightly in thickness, curvature of surface and refractive index. A correction can therefore be applied to allow for the thickness, which will be appreciably the same from one trial case to another; and the power of the lens is at once obtained with the aid of a table from the tape reading. The bench being about 4 meters in length, it is possible to determine the powers of positive lenses up to 3 meters focus, or in trial cases down to .37 D, direct, without the use of an auxiliary lens. For the powers below this an auxiliary lens is employed.

I now come to the question of the accuracy attainable by this method. I should explain that in making a setting the image of the collimator cross-wires is brought into the same plane as the microscope cross-wires—or if auto-collimation is employed, the microscope cross-wires and their image are brought into the same plane—the observer determining the position in which there is no parallax. To a certain extent this gets rid of personal equation. The magnification of the microscope is about 8 diameters. A higher power can, of course, be employed, but this involves a reduced field, which is not so convenient for quick working. The lens can be stopped down, if necessary, to avoid spherical aberration; there is no chromatic aberration, monochromatic light being employed.

Taking the case of a + 1 diopter lens, the error in focusing will not amount to more than 2 or 3 mm., corresponding to an accuracy of $\frac{1}{4}$ per cent. The use of an auxiliary lens may introduce an approximately equal error: for a − 1 D. lens the accuracy may drop to $\frac{1}{2}$ per cent.; *i.e.*, the error may be .005 D. For lower powers the actual error will not exceed this amount; on a .12

lens this is, of course, equivalent to about 4 per cent. On a 3 D. lens an error of 1 mm. in focusing may occur, or about $\frac{1}{3}$ per cent. For a − 3 D. lens the error might then be $\frac{2}{3}$ per cent. or .02 D. For a 10 D. lens the error will probably seldom reach .05 D.; on a 20 D. lens it may easily reach .1 D. In this last case it should be noted that .1 D. corresponds to $\frac{1}{4}$ of a mm. in the focal length.

I should like to attempt to compare the standard of accuracy here indicated with what can be attained by the ordinary method of neutralisation in manufacturing practice. The method consists in forming a neutral combination of the lens to be tested, and a standard of equal and opposite power. Through this combination a distant object is viewed, and if the neutralisation is exact there should be no apparent displacement of the object, as the combination is moved from side to side in front of the eye. If there is any residual lens action a displacement will take place. Let us suppose that the neutralisation is not exact, and that the combination has a power of .01, corresponding to a focal length of 100 meters. If now the combination be moved from side to side in front of the eye, through a distance of 1 cm., the apparent angular displacement of the object will be measured by the ratio 1 cm./100 meters = 1/10,000, which corresponds to about 20″ of angle.

The limit of the resolving power of the eye is usually given as about 1′, corresponding to a diameter of 0.0045 mm. for the ends of the retinal cones. Hering has, however, pointed out that this may easily be reduced in judging of the thickness or of the continuous straightness of a line, and for the average power of the eye in such an observation as this von Rohr suggests that we may take 30″ as the limit. Our .01 combination would then have to be moved through a cm. and a half before the eye could detect any displacement in a straight line.

The method is no doubt a very sensitive one, and capable of giving very good results when employed by a skilled workman, and it is probable that this degree of accuracy can be reached in low-power lenses, *i.e.*, the error need not be more than .01 from a given standard. The method has also the advantage of freedom from chromatic errors, and of compensation for spherical aberration. But, even so, at the higher powers there are other disturbing factors which may come in—want of

accuracy in the surfaces over any considerable diameter, imperfect relative centering of the lens and its counterpart, which diminish the absolute accuracy—and I do not think even a good workman could rely upon being always within 1 per cent., though, no doubt, in the majority of cases he will reach this accuracy.

It is instructive also to consider the effect of errors in the tool or in the working of the lens on the accuracy obtained. If the lens is ground down at the centre of each surface 1/100 of a mm. more than it should be relative to the edge, the consequent error in the power is .05 D. The absolute amount of error is in this case approximately constant throughout the range. Thus, if the error in the low-power lenses is not to exceed .01 D., what I may call the tool error must not exceed 1/500 of a mm., or about 4 wave-lengths of light. No doubt the amount of tool and working error may be expected to be greater in the deeper curves. I am not able to suggest figures as to the degree of accuracy reached with machine tools; nevertheless, it may be useful to bear in mind the numbers I have just given.

Negative Lens.	Positive Lens to Neutralise	Difference from Nominal.	Positive of equal and opposite curvature to Negative.	Difference from Nominal.
—20	18.95	1.05	19.52	.48
—18	17.21	.79	17.66	.34
—16	15.44	.56	15.76	.24
—15	14.54	.46	14.80	.20
—14	13.63	.37	13.83	.17
—13	12.70	30	12.87	.13
—12	11.76	.24	11.90	.10
—11	10.81	.19	10.92	.08
—10	9.86	.14	9.95	.05
—9	8.89	.11	8.96	.04
—8	7.92	.08	7.97	.03
—7	6.94	.06	6.98	.02
—6	5.96	.04	5.99	.01
—5	4.98	.02	4.99	.01
—4	3.99	.01	4.00	.00

I must say a word here as to the standards usually employed for neutralisation purposes. It is, I understand, usual to make nominal + and − 20 D. lenses of such powers that they neutralise one another. It, of course, follows, since the thickness is by no means negligible, that they cannot both be correct. The following table gives the powers of the positive lenses which will neutralise the high-power negatives ordinarily occurring in a trial case. The positive lens is supposed to be as thin as is possible for the usual diameter of about 3.8 cms.; and the thickness of the negative lens at the centre is taken to be 1 mm. The refractive index of the glass may be from 1.50 to 1.52.

It should be noted that the curves of a + 19 D. lens are shallower than those of a − 20 D. lens, which it neutralises, and the positive will therefore not exactly fit into the negative, the separation of the surfaces on the axis being 0.1 mm. The powers of lenses which have surfaces of equal and opposite curvature to those given in the first column are also shown in the table.

It will thus be seen that if it be regarded as essential that the positive and negative lenses of the same powers should form a neutral combination, the powers in the case of the high-power lenses must differ considerably from their nominal values. I do not wish now to enter upon any question as to the advantages or disadvantages of such a system; but, as a question of fact, I am not clear as to whether this system is at present the one generally followed; whether it is usual to make the negative or the positive lens correct, or whether some compromise is adopted; nor, if this system is the one generally followed, whether there may not be exceptions.

It is possible that the rule may be to make the positive and negative tools fit into one another, that is to say, to make the positives and negatives with equal and opposite curvatures. Perhaps definite information on these points may be furnished this evening. I have, of course, only stated facts which are well known to those engaged in the production of trial cases; but one is apt to forget some of the consequences of these facts, and it may be that they are not sufficiently attended to in practice.

All these questions arise in considering the accuracy ordinarily attained in manufacturing practice, and it

is in consequence not altogether easy to arrive at a definite opinion as to the degree of accuracy that can be reached. We have devoted a good deal of attention to the question of the accuracy attainable in test, and have tried various methods in addition to that already described. To one of these I should like to refer shortly. It is a neutralisation method, and therefore necessitates the employment of standards.

A telescope is focused on a distant object, and the neutral combination is then put in the path of the light. Any departure from exact neutralisation will throw the image out of focus; the error can be determined from the distance the eye-piece has to be moved in re-focusing. With a telescope object glass of 60 cm. focal length, an error of .01 D. corresponds to a motion of the eye-piece of $\frac{1}{3}$ of a cm., and can be very easily detected in low-power combinations. In high-power combinations inaccuracies in the surfaces of the lenses affect the definition, but we can read with certainty to .05 D. For comparison with standards the method will probably be useful; the accuracy with which the departure from a standard can be determined will in practice then only depend on the accuracy of the surfaces of the lens to be compared.

A word may be added here as to the use of monochromatic light in the first, or what I may call the absolute method of test. This is necessary for ease and accuracy of working; the difference between the power of a lens for the yellow and the green rays of a mercury lamp will be about $\frac{1}{3}$ per cent.; in the way we use the lamp there may perhaps be a liability to error of half this amount. The powers of standard lenses must, of course, be specified for a definite wave length, preferably that of sodium light or the yellow mercury line.

Centering.—I now pass on to the question of centering. As already explained, on our apparatus this is determined by rotating the lens about its geometrical centre and measuring the displacement of the image. The accuracy of the adjustment for the geometrical centre is tested by focusing on the rim of the lens and rotating.

The distance of the optical centre from the geometrical centre may by this means be determined with an error not exceeding .2 or .3 of a mm. throughout the greater

part of the scale. In the very low powers this accuracy cannot be reached; in a .12 D. lens the error *may* even amount to 2 mm. Other methods, of course, might be employed for the very low powers, but I am doubtful whether any of them could be relied upon to give better results. The method employed in manufacture depends upon observing whether there is any departure from straightness in a line viewed through and outside the lens. This depends on the deviation of rays passing through the lens eccentrically. The amount of this deviation in a .12 D. lens for 1 mm. centering error is 25 seconds of angle, or 1/80th of a prism diopter.

I have already spoken of the limits of visibility, and I am of opinion that practically, for the case I am considering, this error is too small for the eye to detect with certainty. The error of a careful observation would probably not exceed 2 mm. In the practical manufacture of trial case lenses, however, it is scarcely to be expected that lenses can be centered to such a high accuracy, and all the experience I have had goes to show that this error is often exceeded.

It is, I think, useful to note the amount of deviation introduced from lack of correct centering in lenses of different powers. The deviation corresponding to a centering error of 1 mm. is given for certain powers in the following table. The deviation is, of course, directly proportional to the power.

D	Deviation.	
	in minutes of angle.	in prism diopters.
20	68.8	2.0
10	34.4	1.0
5	17.2	0.5
1	3.4	0.1
.75	2.6	.08
.50	1.7	.05
.25	0.8	.03
.12	0.4	.01

It will be seen that in high-power lenses an error of even half a mm. is somewhat serious; while in a .12 lens the error must be very large before it can come to be of any importance, even for the most accurate work.

Axes of Cylindrical Lenses.—The observation for determining the axis error of a cylindrical lens is made at the same time as that for the determination of the power. The collimator cross-wires, the microscope cross-wires, and some cross lines on the lens holder, are all set carefully parallel. The marks on the lens are then set to the cross lines on the lens holder. In determining the power the lens is rotated until the image of the collimator cross-wires is sharp and parallel to the microscope cross-wires. The angle through which the lens must be rotated is the axis error, which is read on the divided circle on the lens carrier.

The axis error can usually be determined in this manner within 0°.3, provided the marking of the lenses is sufficiently good. I have seen marks on trial case cylinders of a thickness corresponding to more than 1°, and the two marks are sometimes not parallel. It may be pointed out that a displacement of each mark of the amount of 0.3 mm. will introduce an error of 1°.

In practice it would seem to be possible to mark the axis with considerable accuracy. In the majority of cases we find the error to be below 1°, except for lenses below 1 D. The error for negative lenses is perhaps rather larger than for positives, and I understand that this is to be expected.

The centering of cylindrical lenses is tested in the same manner as for sphericals.

Prisms.—The method we employ for determining the powers of prisms is the familiar one of observing with a telescope through the prism a scale at a known distance, say, two meters. The prism is rotated through 180° from the one position in which the base apex line is parallel to the scale to the other, and one-half the apparent displacement of the scale divided by its distance gives the deviation in prism diopters. In the case of the higher powers the prism must either be in the position of minimum deviation or a correction must be applied. The mercury lamp is again employed for illuminating the scale; this is a great convenience, as it

avoids the difficulty of reading which would arise with high powers owing to chromatic dispersion.

Lack of definition is due to want of flatness of the faces of the prism, and the method therefore affords a test of the accuracy of the surfaces.

I need not, I think, dwell on the question of the accuracy attainable in test in this instance; practically this is limited only by the goodness of the surfaces. In the low powers the error will not exceed .02 $\triangle$, the prism diopter $\triangle$ being defined as the deviation corresponding to a displacement of 1 cm. at a distance of a meter.

To determine the base apex line of a prism the continuity of a line seen through and beyond the prism is observed; or better, the line is set parallel to the cross-wires in the observing telescope. The error in marking can be determined to about the same degree of accuracy as in the case of the axes of cylindrical lenses. It is necessary, however, that the base apex line should be indicated by two marks at the ends of a diameter of the prism surface. It is a very common practice to put one mark only, and that at the thickest part of the prism, at the point most distant from the metal rim of the mount. An oculist, in setting a prism so marked in a trial frame, is liable to make a considerable amount of parallax error, and it is certainly necessary that the prism should be marked towards its apex line or thinnest part. For verification purposes it is highly desirable that there should be two marks.

It may be remarked here that there are two principal defects of marking of the axes of cylinders or prisms; (1) the line joining the marks may not be in the right direction, and (2) it may not pass through the geometrical centre of the mount. The second defect is the less serious of the two; it can be allowed for in setting the lens in a trial frame by reading the position of both marks. But I fancy this second defect is somewhat frequent, and when only one mark is given it, of course, leads to unavoidable error.

We have found large errors in the marking of the base apex lines of a good many prisms we have examined, often amounting to 3° or 4°. It is possible that a large part of this may be due to the single mark. I am told, however, that it is hardly possible to get the same accuracy in marking for prisms as for cylinders.

Neutral-tinted Glasses.

	PERCENTAGE OF LIGHT TRANSMITTED.	RATIOS CALCULATED.		RATIOS OBSERVED.
A	74.4	B/A	.891	.901
B	66.3	C/B	.769	.766
C	51.0	D/C	.814	.812
D	41.5	E/D	.650	.649
E	27.0	F/E	.737	.757
F	19.9	F/D	.479	.485

Absorption of Neutral Tinted and Blue Glasses.—In determining the amount of light absorbed or transmitted by the neutral-tinted glasses in a trial case, we have up to the present employed a photometer bench fitted with a Lummer Brodhun photometer. For the darker glasses the amount of light transmitted can be measured direct; or we can determine it in terms of the light transmitted by a glass of smaller absorption. The results so obtained for neutral-tinted glasses are quite satisfactory; in a standard set of glasses a complete system of ratios can be observed, and the percentage of light transmitted can be determined with considerable accuracy. I give the results of a short series of observations of this character. (See table in preceding column.)

The case of the blue glasses is not perhaps so simple, nor indeed can the results obtained be interpreted with the same definiteness. Probably the best method is to use a flicker photometer, and compare with standard neutral tints. Practically, however, this question is not of so much importance, and there should be no difficulty in doing all that is required.

For actual verification work we should propose to keep standard glasses at the Laboratory, the absorption of which will be carefully determined from time to time. The absorption of glasses sent for test will be expressed in terms of these standards, the comparison between glasses of approximately the same tint presenting no difficulty.

In conclusion, I should like to express my thanks to Dr. Glazebrook and to Mr. de Graaff Hunter for help given in the work, and also to Messrs. Raphael, Wheway

and Redfern, to Mr. A. E. Grey, and to Mr. W. A. Dixey, for their kindness in lending trial cases and lenses for examination, and for advice on various points.

The Discussion.

Mr. W. A. Dixey: I am sure, Sir, that all of us who have to do with spectacle lenses and trial cases (and there are very few of us present to whom the trial case is not almost a daily tool) will have been very much interested in, and will feel a considerable amount of gratitude to Mr. Selby for his very instructive and interesting paper.

With regard to the National Physical Laboratory's method of estimating the power of lenses, I saw that instrument at the Optical Convention, but not in working order, and, therefore, could not test it practically, but it has never been quite clear to me how the personal equation, as Mr. Selby terms it, is entirely eliminated. Certain factors of the equation are, no doubt, eliminated, but although I may be wrong it always seems to me that when you depend upon the visual estimation of the focus, the question of your own accommodation must come in to a certain extent as long as you are going to judge, as I understand you do judge in that instrument, of the focal position of an image by the focal exactness of the image. Of course there are other methods of ascertaining a focal position, such as the juxtaposition or the identic position of two images of the object as they come together on the focus, and I suppose that Mr. Selby would tell me it depends upon something of that sort.

Mr. Selby's table of differences in the powers of convex and concave lenses for corresponding curves, came to all of us, I expect, with something of a shock. Of course we know it, but the great amount of the differences is to one who has never actually worked it out before (and I do not think any of us have, at any rate, I have not myself) a bit of a shock, and also it comes as rather a difficult problem. What are we to do? We cannot put up with an error amounting to a diopter in a deep lens, or even to one of half a diopter. We must have greater accuracy than that. And, of course, the only way of meeting it—the obvious way to meet it

—is to get your tools not strictly pairs, but to have the curvature of the tools adjusted with a view to the right calculation for thickness, and to assume the natural thickness of the lens.

But even then with deep curves, where the thickness comes in, the difference in thickness of similar lenses, *i.e.*, the thickness of the margin, is a consideration, and, altogether it is a problem which requires thinking about. With regard to the limits in centering, I take the view which I think Mr. Selby expressed—that although one is conscious that an error of 10 mm. in centering a very shallow lens, such as $\frac{1}{4}$ D., with reference to the displacement of the retinal image, has a value which is infinitesimal, and can be easily disregarded, nevertheless, in a set of trial lenses, if they are to be certified as standard lenses, there is no reason why the shallower lenses should not be centered with that degree of correctness which is quite easy, with ordinary care, in manufacture. I do not think there ought to be any difficulty in centering a $\frac{1}{4}$ D. lens, or even an $\frac{1}{8}$ D. lens certainly within 2 mm.—I should have said myself within 1 mm. I certainly do not think a millimeter ought to be impossible, but at any rate 2 mm. would be a possible limit.

I am glad that Mr. Selby has emphasised the importance of marking the axes of cylinders clearly, because that is of enormous importance, and is very loosely done in trial cases one has seen. Mr. Selby has defined very accurately the ordinary errors with which we are all familiar, and that is a matter to which more attention ought to be given. Certainly, too, the marking of the base apex line of prisms is another matter of which I am glad he has emphasised the importance; apex as well as base should be marked, and the marks should be central. In my own case, in making trial case prisms, for a long time now I have put not only those two lines, but also a base line—that is to say, a line marked at the base of the prism at right angles to the direction of the base-apex line, which in the ordinary clinical use of the prism is useful, and helps to set it. But certainly a single mark on a prism is misleading, and very difficult to work with indeed, and at least two marks ought to be used—not on the smaller surface of the prism, but they ought to be on the larger surface

of the prism, which is more or less flush with the rim of the trial lens.

With regard to the difficulty in the accuracy of marking the base apex line on a prism, I do not agree that there is a greater difficulty in doing that than in marking the axes of a cylinder. There is a very good method (I do not know whether it is familiar to everybody) of getting the base apex line of a prism which I have made use of myself in an instrument I have been making quite recently. The method is described in Dr. Maddox's work on the clinical use of prisms, and the great advantage of it is that instead of getting the ordinary displacement angle, you get a very much wider angle—an angle which is three or four times as great. The method is this. You put your prism in a convenient holder, so as to look through it at a point of light, and you see first of all the point of light displaced, as one would expect in accordance with the power of the prism, and then if you look further in the direction of that displacement you will see a sort of "ghost" of the point of light at a very much greater angle than the direct image that you get from the light.

It is a phenomenon which comes from the double internal reflection of the light inside the prism. Practically it amounts to this. You look through your prism, you see your point of light, and then you see another point of light at quite a considerable angle, and by adjusting your prism you can get that point of light, on a plumb line with the direct image of the light, and in that way if you have your carrier properly arranged with a marker adjusted plumb, you can get a degree of accuracy with your base-apex line—I have not measured the degree of accuracy—but I am inclined to say with the marking of the axes of an ordinary prism, you can get a very high degree of accuracy.

Mr. Sutcliffe: Perhaps the lecturer can tell us whether these experiments were taken invariably with the surface of the lens in the same position, either away from the telescope or towards the telescope; and, secondly, was the aperture of the tested lens always the same? I do not know whether Mr. Selby has already told us—he may have done so—but to my mind it would make a difference in the results; in neutralising with

thick lenses, if you "stop down" by using an ordinary stop between the two lenses you very often get different results to what you do if you use the whole aperture of the lens. Again, you get rather a different result if you neutralise positive lenses first on one side then on the other.

Dr. R. Mullineux Walmsley:—The point of view from which I am interested in the paper is that of the Optical Standards Committee, and I am very glad indeed to find that a paper such as this has been presented to this Society, for I think that it may be claimed as consequent upon the labours of that Committee. The result is that, in this paper a very widely-used piece of optical apparatus is taken in hand by trained experts with the object of first examining, and then increasing, the accuracy of the actual parts of the piece of apparatus in such a way as should be appreciated by the trade.

Although I feel some diffidence in making any criticism on the paper, there is one thought which I am inclined to express, and that is this:—One can see from elementary optics that with lenses of high power the lens required for neutralisation is not of the same power, numerically, as the lens to be neutralised, and this suggests a method for utilising the result that Mr. Selby has so ably brought out. The method of neutralisation itself is so simple—so rapid in its application—that one feels that anything which can be done to improve it and make it more accurate is something which is very desirable. The figures that were given us in that table seem to point to this as a practical application, that where neutralisation is very much used you must have for neutralisation purposes a set of lenses marked not with their real powers, but with the powers of what they will neutralise of the opposite sign. Thus, plus 18.95, or something like that, I think was the power to be used to neutralise the minus 20. Thus we require for neutralisation purposes—but for those purposes only, you cannot use them for any other purpose whatever—lenses wrongly marked as to their actual powers, but correctly marked as to the powers of the opposite sign, which they will neutralise. I understood Mr. Selby to say that any actual differences from the thicknesses he gave had but a slight effect

on the result. And, therefore, the suggestion would work that for any thickness met with in practice the lens to neutralise a − 20 would be a + 18.95, and such a lens would be marked for neutralisation purposes as a + 20. I have no doubt the suggestion is a practical one for practical work.

MR. JAMES AITCHISON: Mr. Selby has given us his methods of arriving at results as to the perfection that can be obtained in trial lenses, but I am not quite clear whether he has decided, or whether the Laboratory have come to any decision, as to the margin of error that will be allowed, or whether they have really arrived at any standard upon which the allowance for error in these cases is to be given. It seems that there is really no necessity for much margin to be allowed so that perhaps an absolute standard of perfection may be attained.

In looking at these results also, we have to take the two points of view, that is, the point of view of the Ophthalmic-Surgeon and the point of view of the Sight Testing Optician. Of course, from the point of view of the Ophthalmic-Surgeon who writes a prescription and probably never sees the glasses after they have been made, an absolute standard is very necessary; but from that of the Sight Testing Optician, who measures his results by the actual lenses which he is using, and then, when he has made the spectacles neutralises them from his trial cases, it seems to me that the possibility of error is not so great as in the other case. But at the same time I admit that the standard of optical accuracy ought to be as great for the general purposes of standardisation.

MR. VAL. H. MACKINNEY: I have had a little experience in this kind of work, so that I should like to say a word or two on the subject. Mr. Selby has compared his bench test with the ordinary neutralisation test, and he has based his observations upon the limit placed upon our resolving power. Now, our resolving power—our capability of perceiving two distant sources of light as such—depends upon two things: First, it depends upon the physiological limit placed upon our resolving power, and secondly, it depends upon the natural limit to our resolving power. The fact that light has a *finite* wave-length, and, therefore, that the image of a point source of light is not a point, but a point surrounded by dark

and light bands, places a *natural* limit upon our resolving power. When two distant point sources produce two images such that the first dark band of the one *image* overlaps the central bright spot of the other image, then the angle that the two objects subtend at the eye represents the natural limit placed upon our resolving power owing to the finite size of the wavelength of light, and may be termed the "natural limiting angle of resolution."

Some calculations that I have worked out show that it is possible for this natural limit to exceed the physiological limit. I have drawn up a table which you see here, and which gives apertures for the pupil of 2, 3, and 4 mm., respectively, and taking a mean aperture you find you have a natural limit which reduces your resolving power to 49″. Mr. Selby places it at 30″, and I think an aperture of 3 mm. is rather large for the neutralisation test. The natural limit under ordinary conditions will probably be of the order of 50 to 60 seconds. Now, that is to say, that instead of being able to detect .01 D. we should be only able to detect .02. Thus, in the case of the 1 D. lens you would not bring your test down to 1 per cent. Of course, if it was a 3 D. lens and you could detect a difference of .01 D. the test would be good within $\frac{1}{3}$ per cent.

Now, in the neutralisation test, as I have used it, I think it does get within this accuracy, and for this reason our neutralisation test does *not* depend upon the limit placed upon our resolving power. A moment's thought will remind you of the fact that a person accustomed to neutralisation moves the lenses very rapidly. Now, if he moves them very rapidly he moves that image of a line or cross bar, or whatever it may be, over his retina, and so produces the sensation of light and shade. I do not take it that it is necessary for the light to go from one cone to another in order to produce this sensation of light and shade—the sensation of flicker. I believe that the sensation of flicker is produced if the image only moves sufficiently over the area of one cone, in which case the accuracy obtainable by the neutralisation test might easily exceed Mr. Selby's .01 diopter limit. Of course, the error is so small that in practice we need not worry about it.

Then just a word about tinted glasses. I have had

occasion, of late, to make some tests at the Central Technical College on neutral tinted glasses. I wrote to a firm for some No. 1, No. 2, No. 3, No. 4, No. 5, and No. 6 neutral tinted glasses according to their own standard—the Optical Society's standards were unattainable—and it is interesting to note that their light transmitting powers are as follows: In the case of No. 1 the light transmitted is about 65 per cent; in the case of No. 2, 58 per cent.; in the case of No. 3, 37 per cent.; in the case of No. 4, 23 per cent.; in the case of No. 5, 11 per cent.; and in the case of No. 6, 6 per cent. There is also one other point worthy of notice. I find that the test does not depend upon the brilliancy of the illumination falling upon the photometer screen, but that it does seem to depend upon the separation of the sources. I find that if a separation of about 250 cm. is employed and then reduced down to about 150 cm., the light transmitted is altered (reduced) to the extent of something like 5 per cent.

In testing these neutral tinted glasses I think we should know approximately at what distance the glasses are placed from the sources from which the light is supplied. Then if we have blue glasses tested at all I think the method employed, or proposed to be employed, should be somewhat revised. Might we not say that the light to be transmitted was to be so much, and then say that, beyond wave lengths $4{,}000.10^{-7}$ and $5{,}000.10^{-7}$ mm., the total amount of light transmitted must not exceed a certain definite fraction of the whole. I do not think that it would add greatly to the expense of testing.

Mr. S. D. Chalmers: I would like to raise the question as to how far it is safe or desirable to use the full aperture of a lens in testing without verifying that the focus is the same for a smaller aperture. If the lens is stopped down to an aperture of 5 mm. diameter to correspond with a 5 mm. pupil, I do not believe the focus can be determined with the accuracy which is claimed, and I suggest that the limit of possible accuracy under these conditions should be the standard. Considering a lens of 1 D. there would be, on account of diffraction, a depth of focus of not less than 20 mm. on either side of focus within which it would not be possible to tell which was the best focus.

The beam of light which apparently focuses at a point will form at the focal plane a diffraction disc, the diameter being $\lambda/\sin A$, where A represents the semi-aperture angle and λ = the wave-length of the light considered.

It is easily seen that outside the theoretical cone the behaviour of the light is the same as if this disc travelled parallel to the axis till it reached the boundary of the theoretical cone. The beam of light is thus a cylinder extending $\lambda/2 \sin^2 A$ on either side of the true focus, *i.e.*, for the case cited 40 mm. on either side. At these places the difference in the phase of the light from different parts of the aperture at points on the axis is $\lambda/4$; it is, therefore, certain that at a point half way between this and the focus it is no longer possible to observe the slightest change in the definition as compared with the theoretical focus. There is thus a range of *at least 20 mm.* on either side of focus.

Mr. M. Bentzon: I can only speak of the lens in which I am particularly interested, and in regard to which we have had to employ a different method of ascertaining the exact properties. There is, however, one observation I should like to make upon this subject, and that is this, that it seems to me that a simple system of standardisation would be to use glass with the same definite index of refraction. If you know the index of the glass then the ordinary means of testing the lens, by neutralisation, may be avoided, and I should be very pleased, on some future occasion, to have the opportunity of showing here a measurement instrument which we are using, by which you can measure a lens to within 0.3 diopter. I think that is sufficiently near, according to the remarks I have heard to-night about test lenses in which the nearest approach to accuracy is $\frac{1}{8}$th of a diopter.

Mr. J. de Graaff Hunter: There is only one point upon which I should like to make any remark, and that is the question of the neutralising method of testing negative lenses. Of course + 20 and − 20 do not neutralise. The neutralising method of test is a very good method, at any rate for making a lens. It is a very good method of testing when the lenses arrive at a proper figuring, but I do not think it would be necessary in order to check the negative lenses to have a complete system of positive lenses which were not cut to the

standard dimensions already supposed to be in the trial case. For instance, a − 20 and a + 20 lens, of course, if truly made, do not give a neutralising effect when placed before the eye, but if they are combined also with a − 1 lens, they will give practically perfect neutralisation. So by using three lenses instead of two, it will be quite possible to get the advantages of the method without departing from accuracy in the formation of the positive series of lenses. That would allow in all cases accuracy to be reached to the limit of the lowest power lens in the trial case. For instance, one always will be able to get to at least .12 D. by that method. I do not know if that accuracy is considered quite sufficient, but at any rate it would not be necessary to duplicate all the positive lenses in order to get lenses which would neutralise the negative lenses.

Mr. Dixey: Allow me to point out in two words that there are two other practical methods of getting better results than the results you get by putting a + 20 and a − 20 together. One is, if you want to neutralise a + 20 lens you take two − 10 lenses and put one on each side of your + 20 lenses. The other method I am inclined to think is the best of all, and that is, to have all your trial lenses worked with one surface plane, and then if you put the two similar lenses together you ought to get, as far as I can see, perfect neutralisation.

Mr. H. S. Ryland: It appears to me that one of the most important points raised in Mr. Selby's paper is to determine exactly what your constant shall be, since in the higher powers, the thickness of your lens becomes an important factor. In the trial case our powers are increased in definite steps. Hence, a few millimeters difference in focal length does not matter, provided that this difference is known. I would suggest, therefore, that from the point of view of general convenience, the trial lenses should still have the same nominal powers, but with the curves equal and opposite. Of course, these would be standardised and the equivalent focal length known. This method would save the necessity for two sets of tools, and, moreover, the lens measure could still be conveniently used, as the curvature of the lens would be proportional to the nominal power.

The President said: In the first place, as regards the use of the D.-line of sodium light as the standard for

this work, it really seems to me quite immaterial what standard is used, provided everyone agrees to use the same. But for my part I should like to suggest the use of the green mercury light rather than the sodium light, because I think it is nearer the maximum visual intensity in the spectrum. The use of the D. sodium line is not really quite satisfactory; it is rather too far towards the red end of the spectrum, and the use of the E. line, which is really the mercury line, has been suggested as the standard for the determination of refractive indices rather than the D. line. Further, as far as I can judge from the appearance of the light, Mr. Selby's mercury vapour lamp gives a good deal more green light than sodium light, and it might be easier to cut out the sodium light than the green.

Then as regards the question of determining the exact position of the focus, I could not help calling to mind, when that point was raised, the device which Mr. Sutcliffe showed here two meetings ago, by which it was quite possible to note definitely when focus was attained by the complete over-lapping of two images. I do not know whether that device can be applied to these measurements, but if that can be done, it would eliminate the personal equation, although I imagine that the superposition of the image and the cross-wire can be done with a considerable degree of definiteness.

Mr. F. J. Selby: I should like, Sir, to thank you and the Society, and especially Mr. Dixey and Mr. Sutcliffe, for the kind way in which they have received my paper. I am afraid I can hardly reply in detail to all the criticisms which have been made. But I should like to begin by referring to one matter which has not been raised, and which I hoped might have been referred to, and that is the question of what actually is the practice at present in making lenses. It would be extremely useful both with a view to decide what is to be done in the future, and with the idea of getting a clear and definite notion of what is the accuracy that can be reached in practice to know what is actually done with regard to the curvature of lenses, whether positive or negative lenses are made with equal and opposite curvatures, or whether they are made to neutralise.

Mr. Ryland: I think they are generally made with equal and opposite curvatures.

MR. SELBY: That is what I have believed to be the case. However, the neutralisation method is employed in testing, and it is clear that if made with equal and opposite curvatures the lenses will not neutralise, and that allowance for that has to be made in the method of test.

MR. DIXEY: May I interrupt to say that the tools are equal and opposite at the beginning, but in many workshops, I believe, they are not so before the test is completed? Then there is another quite practical point, and that is this: that a man in working his tools in so far as they are hand-worked has a power of varying the curvature. What sort of curve he gets on his surface I do not know, but he can put depth on his curve by working nearer the margin of the tool, and *vice-versâ*, and in that way he satisfies the requirements of whoever it is who is going to conduct the neutralising afterwards.

MR. SELBY: It is at any rate a considerable help to know that what is being aimed at is equal curvatures. So far as I have been able to ascertain from actual measurements there is a very considerable amount of divergence between different high-power lenses of the same nominal power, and I inferred from the results that probably what was being aimed at was equal curvature; but there is variation both on one side and the other, and the departure from neutralisation is in some cases very large indeed. To refer now to some of the points that have been raised, Mr. Dixey spoke in regard to the question of focusing in Dr. Drysdale's apparatus or our own, of the difficulty there always is in deciding the exact focus and the amount of personal equation that may come in. And the point that Mr. Chalmers made might perhaps be mentioned in the same connection. I would point out that we do not depend exactly upon focusing in the ordinary sense in determining the position of the image; we judge by the absence of parallax. What we do is to get the cross-wires in front of the microscope, and the image of the collimator cross-wires side by side and in the same plane. Regarded as focusing in the ordinary sense, both cross-wires must be focused together for the eye, and in judging the focus for both simultaneously there is not very much liability to personal equation.

When one is observing by means of parallax I do not think that personal equation comes in to anything like the same extent as in ordinary focusing. At the same time I do not think it is entirely eliminated, but I think the limits of error I have stated may be taken to include whatever personal equation remains when one is adopting the parallax method. With regard to Mr. Chalmers' point, that is certainly more difficult, and I am not clear as to what effect that would have when one is employing the parallax method. I have not actually worked on a 1 D. lens for an aperture of 5 mm. I have worked with an aperture of 1 cm., and I take it that would bring down the distance for the range over which the focus extends to a quarter of the amount which Mr. Chalmers stated. I think he said about 30 mm. to 40 mm.

That brings it down to from 8 to 10 mm. I think in working with the parallax method the difference between two readings would not be anything like that amount. I am not at the moment prepared to say why one should fix upon one position of the cylindrical part of the pencil any more definitely than another, but my experience is that one is able to see very quickly a change in the parallax effect.

I do not find as a matter of practice that errors arising from this cause are as great as would be inferred from what Mr. Chalmers has said. Of course, we work with an aperture which is larger than is needed in the actual use of the lens. That in itself is a defect of the method, unless we suppose that the lenses are sufficiently accurate over the larger surface. I think in the majority of lenses that is the case. Judging from the actual results of measurements of trial cases the results come out extraordinarily good in a very large majority of instances. I do not think it is necessary, practically, to consider the question of stopping down in the lower power lenses. Of course, in the higher power lenses it is necessary on account of spherical aberration, and the auto-collimation method, as we employ it, practically ensures that we only use the centre of our lens.

As to the errors that occur in the high-power lenses under the present neutralisation method of test, Mr. Dixey has spoken of that, and of the question as to the standards to be employed, to which Dr. Walmsley also referred. Of course, I intentionally abstained from

going into the question of what it would be desirable to lay down in regard to standards. It is a matter that is still before the Optical Standards Committee, and I do not think it would be advisable for me to discuss it until they have come to a decision on the point. I may, however, just say this. It is perhaps possible, from a practical point of view, to work on either method. You may make your + 20 D. lens correct—actually with a power of + 20—or you may make it of such a power that it will neutralise a − 20. So long as you choose one or the other of these alternatives definitely, it is quite possible to construct a working system which will be satisfactory in most respects. From many points of view there is a great deal to be said for the neutralisation method. Whichever method you choose, there is, I take it, no difficulty whatever in turning out the lenses, with the accuracy necessary, to the particular standard. Of course, if you say + 20 is to be + 20, then the standard negative lens used to neutralise it will not be a − 20; you will have to use a special standard for the neutralisation test. The necessity of having to use special standard test lenses is, no doubt, one of the disadvantages of adopting what I may call the accurate method. The positive and negative lenses in a trial case can no longer be tested against each other.

As regards the centering in low power lenses to which Mr. Dixey referred, my statement as to the possibility of a 2 mm. error in the centering was, of course, made for a $\frac{1}{8}$ D. lens. If you go to a $\frac{1}{4}$ D. lens I should think it is quite possible to see the 1 mm., so far as I am able to judge from my own observation, if care is exercised. I am quite clear, however, that larger errors than that often occur; as much as 3 or 4 mm. in a $\frac{1}{4}$ D. lens. This amount of error is quite visible whatever method of test be adopted, but such errors do occur, and I do not know to what extent they are to be put down to mere carelessness, or whether one must regard them as unavoidable, even in the lenses that are to be used for testing purposes as lenses in trial cases are used. Another matter to which Mr. Dixey referred is the marking of prisms. He suggested that the marking ought to be on the other face—the face which is flush with the rim. Is it not rather a difficulty that that face will be next the trial

frame, and that it may not be possible to see the mark in setting?

MR. DIXEY: The mark should be visible through the other surface.

MR. SELBY: Yes, but in one of the thick prisms might it not be a little difficult where the base is bevelled? I certainly think that this face is the proper place on which to mark, if this difficulty does not arise.

MR. DIXEY: If the marks were true, you could rely upon the mark at the other end, and then the base line I suggest is always visible. If they are put into a trial frame with a central mark upon it, you could set your prism by means of the mark on the edge of the prism. If they are set into a frame without any scale, as they often are, you can be guided by the base-line quite easily —not the base apex-line, but the line at right angles.

MR. SELBY: Quite so. For all other reasons I should think it best to mark prisms as Mr. Dixey suggests, but I judged from the fact that all the prisms I had seen were marked on the other face, that the difficulty I have mentioned had to be avoided. With regard to the accuracy of marking the axes of prisms, I also do not see why they should not be marked as accurately as cylinders. I hoped that perhaps some explanation of the greater errors in prisms might be given. The method which Mr. Dixey suggested of getting the marking for prisms is, I think, of no little interest. If it is not getting too late I should like to say just a word about that. I happened to be looking into it the other day, and I think, from an observation of the deviation obtained in the ordinary way, and the deviation obtained with the double reflection by the method Mr. Dixey suggested, one could get the angle of the prism as well as the deviation.

MR. DIXEY: To get the power of the prism you would have to take the index, I think.

MR. SELBY: If the observations be made on a scale, and the displacement due to the ordinary deviation be observed, and at the same time the displacement due to what I may call the double reflection deviation, then both the index and the power can be determined. If I call D_1 D_2 the two deviations respectively, then the formulae giving these are—

$$(\mu - 1)\, i = D_1$$
$$(3\mu - 1)\, i = D_2$$

where i is the angle of the prism. From these equations we can determine both μ and i. The difference between D_1 and D_2, viz., $2\ \mu\ i$, is the angular separation of the two images in Mr. Dixey's method, and it is this amount of separation, which is about six times the ordinary deviation, which gives to that method its sensitiveness.

Mr. Sutcliffe asked about the setting of a lens, as to which face was towards the observer. In general, we set the lens in one position first, and then reverse it. That is often necessary with negative lenses, which are frequently mounted unsymmetrically, and you have to take two observations to get a measurement from the lens centre. Mr. Aitchison referred to the question of the limits of error to be allowed. That, of course, is a matter which is being considered by the Standards Committee, and, therefore, I did not wish, and do not wish now, to say anything about it this evening. The limits of error which will be allowed have not been yet definitely laid down by the Standards Committee; they are under consideration, and I hope that this matter may be dealt with shortly. But, of course, the question as to the true powers of the high-power lenses, which has been discussed a good deal this evening, raises a point that must be dealt with before the limits of error can be finally laid down. Then there is the point raised by Mr. Mackinney.

With regard to the resolving power, and the possibility that the effect on the eye, in the ordinary neutralisation test, is really a flicker effect, and not an actual displacement—not a mere effect of motion—is quite an interesting one. I do not know that it affects my argument in the paper at all appreciably, but it is distinctly interesting from a theoretical point of view, and I should like to think about it a little before I say very much in reply. But in any case, of course, it is the motion of a line that is being observed, and I quite think that it may be possible to observe that motion at a much smaller angle, if I may so express it, than one could observe a discontinuity in a line. It is, however, very difficult to say what amount of motion it would be possible to observe. I am not clear that the question of resolving power comes in very much. If the line is thick, relatively speaking, you may still be able to observe the motion of it if you move it from side to side, and I

should be inclined to say, accepting the usual physiological theory, that you would have to over-step the single cone to observe the motion. But I do not in the least lay any stress upon that, and, of course, it may very easily be quite wrong, and it may be, as Mr. Mackinney has suggested, that the flicker effect is perhaps of more importance, from the point of view I have been considering, than the actual effect of the motion, but in any case it would be extremely difficulty to separate the two effects.

As I say, however, I do not think it affects my argument appreciably. I did not intend to lay any great stress upon the limit I suggested of 30″—that was merely a rough average limit, and in some cases, no doubt, it is possible, in judging of the continuity of a line, to work down to a much lower limit than that—10″ perhaps. I do not know whether the diameter of the retinal cones is known with any great accuracy. I have put down .0045 mm. in my paper, as corresponding to one minute of angle, and suggested that that was about the diameter of a cone—the figure is given by von Rohr. I think Mr. Dixey himself in a paper which he read before the Optical Society gave .001, a quarter of the above figure. There is no certainty, at present, in these figures, but I was only suggesting what seemed to fairly represent, in practice, the facts of the case.

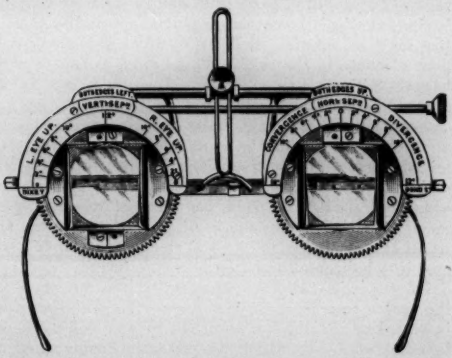
Mr. Ryland suggested that it would be desirable to work the lenses to equal curvatures, and, of course, that brings up the powers of the high-power lenses very much more nearly to their nominal values than working on the neutralisation basis. However, that is a matter which, as I have already said, I do not wish now to discuss. Our President referred to one point which I will remark upon—the use of the green light of the mercury lamp as the standard in the definition of the power. It may be that would be more convenient. Of course, I suggested that sodium light should be used because that has been accepted as the standard for so long, and in this particular instance it really does not, I think, practically matter at all which you choose. It is only necessary to specify one definite wave-length. Of course, in the ordinary practical neutralisation method of test it makes no difference what light is used. The combination is neutral for all colours.

Just one point further. Mr. Dixey spoke of special combinations that might be used for neutralisation purposes. I do not think that is necessary. I think it is just as satisfactory, and more simple, to use one single lens to neutralise. You have only to decide what the power is to be, and I think one single lens will neutralise as well as two plano lenses—plano-concave or plano-convex, as the case may be—practically just as well, I think, if made of the correct power.

TO MEASURE FUSION POWER OF EYES.

By W. A. Dixey.

Half an hour is, I understand, the time allowed for the introduction and discussion of my subject. It is little enough, but I think it will suffice for presenting to your consideration an ophthalmic instrument which I have recently made to the design of Dr. Maddox, and which, though it does not embody any new optical or mechanical principle, may, I think, be not entirely without points of interest to some of my fellow Opticians. The instru-



ment is called "The Prism Verger"; and if you allow me the use of the word vergence as a generic term to cover the four kinds of vergence, viz., convergence, divergence, right supra-vergence, and left supra-vergence, the word verger will naturally follow to denote an instrument whose function is to induce vergence of the ocular axes.

To appreciate the use of the instrument you should understand that it is a necessary condition of binocular sight that, when the two eyes are directed to a distant

object, the visual axes should be parallel. The Prism Verger is a device for increasing the angle of deviation from the parallel, firstly, in the horizontal direction, and secondly, in the vertical direction. These two involve all directions, for an oblique direction can always be resolved into its horizontal and vertical elements.

If you hold a prism, say of 6° deviation, with its edge up in front of your eye, and look through it at a distant object, the object will be apparently displaced in a vertical upward direction. If you now rotate the prism through a small angle, so that its edge points not vertically upward, but in an oblique direction, say 10° from the vertical, the object will still be displaced to the same extent as before, but in a direction corresponding to the angle of rotation of the prism.

This oblique displacement can be expressed as a vertical and horizontal displacement, and you will see that the horizontal displacement is proportionate to the sine of the angle of rotation, and the vertical displacement is proportionate to the co-sine of the angle. This is what happens with the Prism Verger. When the prisms are in the zero position with both edges up, the visual lines are deflected equally and in the same direction, and the effect upon the vergence of the eyes is nil. By turning the milled head we rotate the prisms in opposite directions with a result that, while the vertical deviation remains equal and similar in phase for the two eyes, the horizontal deviation is opposite in direction; so that the net result is a graduated effect upon the vergence of the two eyes in a horizontal direction only.

For the chief factor in determining the alignment of the ocular axes is an intolerance of the confusion that is caused by the duplication of the objects looked at, or, in other words, the desire to maintain fusion; and as by the rotation of the prisms the visual lines are gradually deflected from the parallel, the desire to maintain fusion will compel a corresponding alignment of the ocular axes. It is obvious that in this way the limits of the power of fusion in the two horizontal directions may be ascertained. Similarly by adjusting the prisms with both edges pointing horizontally in the same direction, so that the horizontal deflections are equal, and then by rotating them as before, we can measure the fusion power in the vertical directions.

This measurement of the fusion power of the eyes should not be confused with the measurement of heterophoria, or the static balance of the ocular muscles. That also is a useful measurement in sight testing. But the measurement of fusion power affords a safer and more useful pointer when it comes to a question of treatment for muscular insufficiency.

The treatment of muscular insufficiency offers some of the most difficult problems in ophthalmic practice. The correction of defects of accommodation and of refraction is more or less subject to measurement and rule, but I suppose that the proper treatment of defects of vergence is more than any other dependent upon medical knowledge and experience, knowledge which ensures a correct diagnosis of the cause of the defect, and judgment founded on experience which indicates its most advantageous treatment. For there are always two ways of treating a weak muscle; one is to rest it, and the other to exercise it; and those two methods are, as applied to vergence defects, exactly opposite.

I would have you notice that this instrument is equally adapted to either treatment. It is obviously useful for exercising the muscle in the reverse direction to the weakness; and if the relief of the weakness is indicated by the conditions of the case, the fusion area may be measured under the desired condition of accommodation, and a prismatic assistance applied in accordance with a recognised rule. I venture, however, to caution the younger members of the Society that it is not safe to recommend either of these methods of treatment without medical sanction and supervision.

Finally, the instrument may be used with a Maddox rod to measure heterophoria, and if one prism be slipped out of its setting and the other set edge up, it will measure, with a tangent chart, the latent excess or insufficiency of convergence at a reading distance.

THE EFFECTS OF DISTANCE ON HIGH POWER SPHERO-CYLINDRICAL LENSES.

By S. D. Chalmers, M.A.

Mr. S. D. Chalmers, M.A., opened a discussion on "The Influence of Position of the Object on the Effective Value of High Power Sphero-Cylindrical Lenses."

It is well known that the effective value of a lens is altered when its distance from the eye is changed, and that this effect is relatively greater with higher power lenses. In the case of sphero-cylindrical lenses such effects will be different in the two meridians, since the powers are different, and the effect of altering the distance will be to alter not only the spherical power of the equivalent lens (in contact with the cornea), but also its cylindrical power.

Effects of this kind must be familiar to all users of the retinoscope and the ophthalmometer, and to all who are in the habit of fitting high-power sphero-cylindrical lenses; but it does not appear to be commonly known that the position of the object has a considerable influence on the equivalent cylindrical, as well as spherical power, and that serious errors may arise from neglecting this effect. In fact, with high-power sphero-cylindrical lenses the same lens is not suitable both for distance and for reading, even though the accommodation is normal.

In the following table I have evaluated the effective value at the cornea of lenses of different powers, when used for a distant object and an object at 250 mm. from the lens, which is taken at 15 mm. from the cornea. Taking a + 2 D. lens its focus for distant objects will be at 500 mm.; it will, therefore, bring light to a focus at 500 − 15 mm. = 485 mm. behind the cornea, and is, therefore, equivalent to a lens of 485 mm. focal length in contact with the cornea, *i.e.*, a 2.06 D. lens.

TABLE.

Power of Lens.	Equivalent Lens at Cornea. Object		Power of Lens.	Equivalent Lens at Cornea. Object	
	At infinity	At 250mm. from lens.		At infinity.	At 250mm. from lens.
+ 1	+ 1.02	+ .90	— 1	— .98	— .88
+ 2	+ 2.06	+ 1.88	— 2	—1.94	—1.78
+ 3	+ 3.14	+ 2.79	— 3	—2.87	—2.57
+ 4	+ 4.26	+ 3.77	— 4	—3.77	—3.37
+ 5	+ 5.40	+ 4.79	— 5	—4.65	—4.16
+ 6	+ 6.60	+ 5.88	— 6	—5.50	—4.98
+ 7	+ 7.82	+ 6.91	— 7	—6.34	
+ 8	+ 9.09	+ 8.03	— 8	—7.14	
+ 9	+10.40	+ 9.17	— 9	—7.98	
+10	+11.77	+10.37	—10	—8.70	

In using the table for sphero-cylindrical lenses, the actual powers in the principal meridians are used. A + 4 D. sph. ⌒ + 4 D. cyl. has powers + 4 and + 8 and is thus equivalent to 4.26 and 9.09, or + 4.26 sph. ⌒ + 4.83 cyl. at the cornea. If the distance were 20 mm. instead of 15 mm. the cylindrical effect would be 5.17 instead of 4.83, a change in the position of the lens of 5 mm. producing a difference in the equivalent cylindrical effect of .34 D. With higher powers the difference would be still greater.

This result shows the necessity of specifying the distance of the lens from the eye when high-power sphero-cylindrical lenses are used and abnormal distances required.

Near Objects.—If the object be assumed at 250 mm. from the lens + 4 D. of the power in each meridian is used up in making the light parallel, and the light converges after passing through the lens as if the power in each meridian were reduced by 4 D. and the light came from infinity. Thus, in obtaining the effect of the distance, we must add — 4 D. to the existing power in each meridian and obtain the equivalent of this lens from column two of our table. The lens so obtained would make light from infinity focus in the same way as the actual lens makes the light from the near object. To obtain the actual equivalent lens we must add on in each

meridian sufficient power to make the light from an object at 250 + 15 mm. = 265 mm. parallel, *i.e.*, a power of 3.77 D.

Thus, for + 6 D. we have 2.06 + 3.77 = 5.83 D.

And for − 6 D. we have − 8.70 + 3.77 = −4.93 D.

Thus a lens of powers − 2 and − 6 (− 2 D. sph. ⌒ − 4 D. cyl.) has equivalent powers for a *distant object* − 1.94 and − 5.50, *i.e.*, − 1.94 sph. ⌒ − 3.56 cyl., and *for an object at 250 mm.* − 1.83 and − 4.93, *i.e.*, − 1.83 sph. ⌒ − 3.10 cyl. The spherical difference is unimportant, since it requires a small change of accommodation or variation of the object position, but the cylindrical difference is very important; in fact the lens, which is most suitable for distance, is not satisfactory for reading, even if the accommodation is good.

In the case of a + 4 sph. ⌒ + 3 cyl. the cylindrical effect is 3.56 for distance and 4.12 for reading, while in the case of a + 4 cyl. the difference is .49.

I have brought this matter before you hoping that it might explain some difficulties which occur in practice, and that some members might be able to explain how the difficulties that I have mentioned are minimised or avoided in actual practice. It seems to me that this may explain some of the difficulties in connection with astigmatic accommodation.

Mr. Dixey said he had been considerably impressed by the importance and interest of Mr. Chalmers's figures, which had a distinct application in ordinary ophthalmic work.

Mr. Val. H. Mackinney said that the subject which Mr. Chalmers had brought before them in such a new light was of very great importance indeed. The matter had, he thought, been realised by members who practised Sight Testing, but he did not think its importance was sufficiently realised. He had little doubt that the fact was generally known; the actual effect, however, in practice was not understood. He had that evening roughly plotted the effect of this alteration in the accommodating power of the eye with a lens of power sph. + 2 ⌒ cyl. + 2 axis vertical placed at a constant distance from the eye. The curve obtained seemed to put the matter very clearly and enabled one to realise the great importance attached to the fact. He said that he would very much like to see a complete paper on the subject from Mr. Chalmers.

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